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WORD

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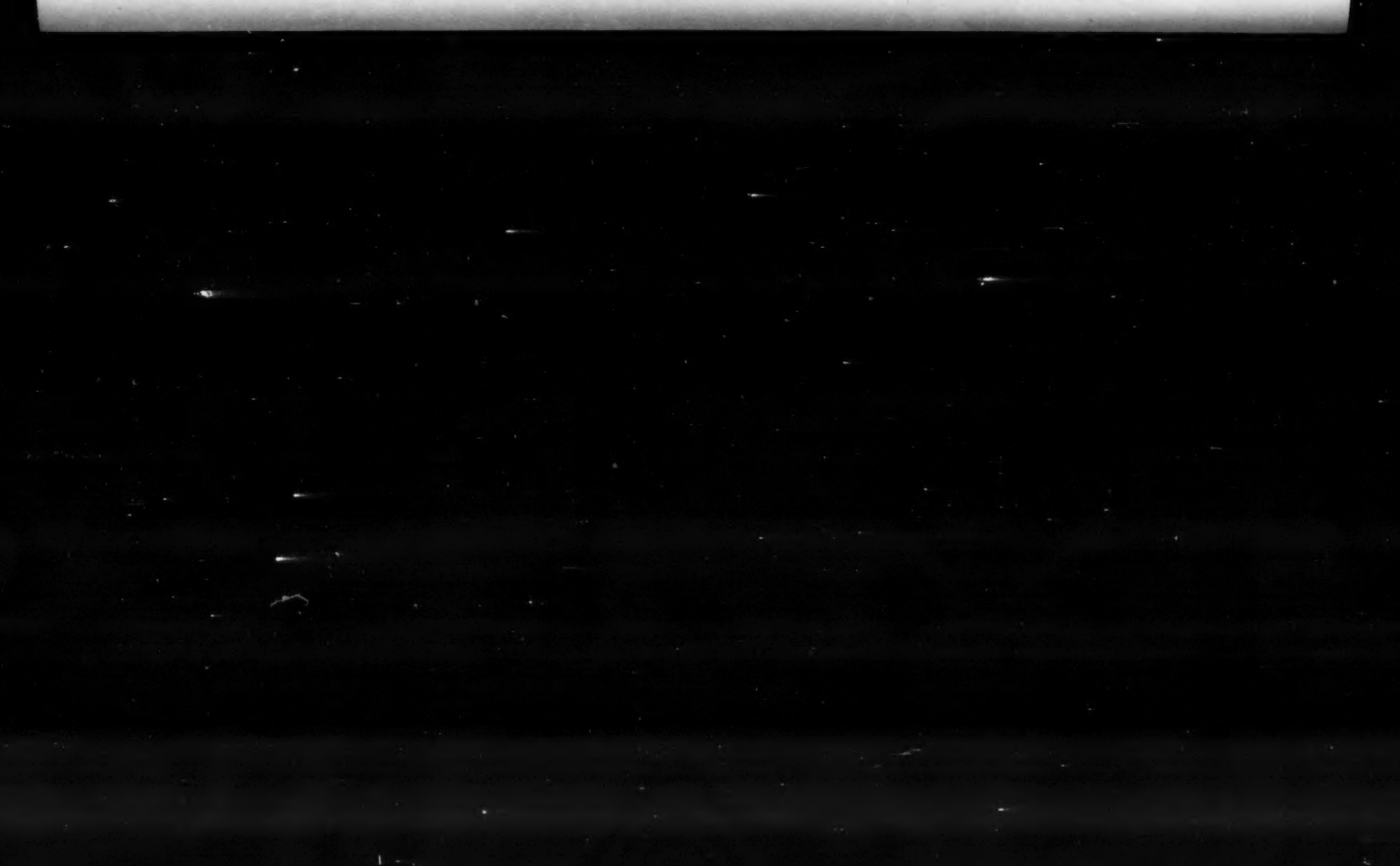

WORD

VOLUME 14 *August-December 1958* NUMBER 2-3

A THEORY OF PITCH ACCENT IN ENGLISH*

DWIGHT L. BOLINGER

1. THE FUNCTION OF PITCH

Like most other languages, English has contrasts of pitch. Unlike the pitch contrasts of certain languages, the English contrasts are not "phonemic" in the sense that raising or lowering the pitch of one syllable in a word such as *tower* will change its meaning in the way that replacing /t/ with /d/ will change it, causing it to point to something completely different in the world beyond language.

This much pitch in English does not do. No other phenomenon in language, however, has more firmly resisted efforts to find out what it does do. In the last two decades, linguists in the United States have attempted to take the garrison by main force. They have based their operations on a set of propositions which may be regarded either as assumptions or as conclusions from the evidence, depending on how far the evidence is credited. Among these propositions are the following:

1. That pitch functions in the same way as the segmental phonemes—the vowels and consonants—and that a sequence of different pitches will produce something potentially meaningful in the same way that a sequence of vowels and consonants may produce a word. In order to manage this, it is necessary to decide when one pitch is different from another. The segmental

* Research at Haskins Laboratories, 1956-57, supported by a grant from the Carnegie Corporation of New York. The author gratefully acknowledges the cooperation of the Laboratory staff, particularly the hand-drawing of spectrograms by Prof. Pierre Delattre and the technical and editorial help of Dr. Louis J. Gerstman.

phonemes can be distinguished with relative ease because they are complex: each contains certain "distinctive features," whose presence or absence rather sharply separates one from another. Pitch has only one ingredient, the fundamental frequency of the voice. As a speaker moves up or down the normal range, there is no point at which the ear can detect that one thing ceases to occur and another begins—there is only up and down. So it is impossible to isolate a pitch as one can isolate a segmental phoneme: an untrained listener can identify a prolonged English /s/ without benefit of context; no amount of training will enable him to distinguish one linguistically significant pitch from another, however much prolonged, unless it occurs in context. Relying on contexts chosen for the purpose, analysts have discovered, or invented, "contrastive levels," each level representing a phoneme. Four is the usual number of levels.[1]

2. That the meaningful something constitu ed by pitch phonemes is an intonation morph. The morph *231* (numbers refer to the "contrastive levels" of pitch) differs from the morph *221* as *tower* differs from *bower*. For those who follow Smith and Trager[2] the morphs are more complex, including, besides the pitch levels, "terminal junctures," which are ways of going from phrase to phrase or from phrase to silence, and which involve both pitch (direction, not level) and tempo. Such an intonation morph would be *231* $\#$, which can be described phonetically as "mid-low level followed by mid-high level followed by low level followed by

[1] Zellig S. Harris, in *Language*, XX (1944), 189, marked seven. Rulon Wells, in *Language*, XXI (1945), 27-39, marked four, as did Kenneth L. Pike in his *Intonation of American English* (Ann Arbor, 1945). George L. Trager and Henry Lee Smith Jr., in their *Outline of English Structure* (Norman, Okla., 1951), have followed Pike, and recent texbooks based on the *Outline* have propagated the number four. (For advanced classes: Charles F. Hockett, *A Course in Modern Linguistics* [New York, 1958]; A. A. Hill, *Introduction to Linguistic Structures* [New York, 1958]; Henry A. Gleason, *An Introduction to Descriptive Linguistics* [New York, 1955]; Velma Pickett, *An Introduction to the Study of Grammatical Structures* [Glendale, Calif., 1956]. For beginning college classes: Donald J. Lloyd and Harry R. Warfel, *American English in Its Cultural Setting* [New York, 1956]; W. Nelson Francis, *The Structure of American English* [New York, 1958]; Paul Roberts, *Understanding English* [New York, 1958]. At least one for high school: Paul Roberts, *Patterns of English* [New York, 1956].) There has been no serious attempt to collect a real corpus of examples to test the theory, nor has it been proved experimentally. For intonational contrasts that the Trager-Smith *Outline* fails to account for, see my "Intonation: Levels versus Configurations," *Word*, VII (1951), 199-210.

[2] *Outline*, especially p. 46.

terminal fall and fade to silence, with a slowing down toward the end."[3]

3. That pitch and stress are phonemically independent. It is recognized that changes in stress may affect any level of intonation that happens to be running at the moment, but not, for example, in such a way as to raise a Level 2 pitch to a Level 3 pitch; the changes are phonetically slight and phonemically non-distinctive.

My purpose in this article is to deny the third assumption, and to reverse the roles of stress and pitch. I shall offer evidence that far from being a non-distinctive by-product or a completely independent variable, pitch is our main cue to stress.

2. Earlier Notions of Pitch as a Cue to Stress

The idea that stress may depend on pitch is not new. The experiment of John Muyskens in 1931, using kymographic records, purported to show that the familiar noun-verb pairs like *pérmit—permít* are distinguished by higher pitches on their stressed syllables. Kenneth L. Pike[4] and Daniel Jones[5] demolish this argument by pointing out, in Jones' words, that "it often happens in a language that strong stresses are found on low-pitched syllables and weak stresses on high-pitched syllables." To demonstrate this, all we need to do is turn the *permit* example into a question: *pérmit?*

The refutation, however, is based on a persistent fallacy: that in order to serve as a cue to stress, pitch must RISE. Recent discussions and descriptions continue to look for this kind of relationship,[6] and, failing to find it, enter a verdict against pitch in general.

[3] The analysis of Spanish juncture and intonation made by Stockwell, Bowen, and Silva-Fuenzalida, *Language*, XXXII (1956), 641-665, which follows Smith and Trager, throws some doubt on the morphemic status of intonation patterns: "A sequence of pitches up to and including a terminal juncture will be referred to as an INTONATION PATTERN. Whether or not such a sequence of suprasegmental elements is a morph remains to be demonstrated on the morphological level of analysis" (p. 661). It is hard to see on what basis the levels and junctures are contrastive units, if sequences of them are not morphs.

[4] *Intonation*, pp. 16, 83.

[5] *An Outline of English Phonetics* (New York, 1956), § 912 footnote.

[6] Compare G. F. Arnold, "Stress in English Words," *Lingua*, VI (1957), 226, and K. L. Pike and W. Kindberg, "A Problem in Multiple Stresses," *Word*, XII (1956), 421.

The experiments that I shall report suggest that it is not pitch RISE, but rather pitch PROMINENCE, that is essential to what we react to as stress. By prominence I mean a rapid and relatively wide departure from a smooth or undulating contour. A rise is only one kind of pitch prominence, though it is certainly the commonest kind. In the following simplified diagrams, the "corners," or "sharp points," represented by dots, indicate some of the ways in which a syllable can be made to stand out by means of pitch:

3. Stress and Intensity

According to Bloomfield, "*stress*—that is, intensity or loudness—consists in greater amplitude of sound waves."[7] Jones gives more attention to gestural accompaniments, but he too insists on "the objective impression of *loudness*."[8] Nearly all linguists

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The latter deals with a complex relationship of stress and pitch in Campa; the wording on the page cited indicates that the investigators gave up associating pitch with stress when it became apparent that HIGH pitch was not systematically related to it. If applied to English—I cannot judge its application to Witoto—the following statement would illustrate the current fear of mixing levels: "There is a complex suprasegmental phoneme of stress accompanied by high pitch" (Eugene A. Minor, "Witoto Vowel Clusters," *International Journal of American Linguistics*, XXII [1956], 137)—rather like saying that the dawn breaks accompanied by sunshine. Uriel Weinreich draws my attention to the following from N. I. Žinkin, "The Perception of Stress in Russian Words" (in Russian), *Izvestija Akademii pedagogičeskix nauk R. S. F. S. R.*, LIV (1954), 7-82, page 9: "The claim that under stress the fundamental pitch of the vowel is raised, is subject to doubt.... This raising should be attributed to sentence intonation, not to word stress. A pronunciation is possible without such raising. Furthermore, whispered pronunciation is possible, where the fundamental pitch ...is excluded altogether." Again pitch is dismissed as a cue to stresss a a result of failure to find a pitch RISE. It is reasonable, of course, to assign to the role of sentence intonation, instead of to stress, a particular manifestation of pitch change, e. g., rise rather than fall. If, however, we find that either a rise or a fall, when certain conditions are met, is responded to as "stress," then we have pitch playing a dual role; I have preferred to keep the two functions intact, labeling them *accent*. (As for the absence of intonational contrasts in whispered speech, see my speculation on a change of vowel quality to substitute for it, *Studies in Linguistics* V [1947], 77, confirmed experimentally by Werner Meyer-Eppler, "Realization of Prosodic Features in Whispered Speech," *Journal of the Acoustical Society of America*, XXVIII [1956], 760.) These comments, of course, are beside the point if Russian stress and intonation are essentially different from English.

[7] Leonard Bloomfield, *Language* (New York, 1933), § 7.3.

[8] *Outline*, § 909.

have followed this lead.[9] The unanimity of their opinion is a roadblock that must be got out of the way before an explanation based on pitch can gain acceptance. The removal will occupy a large part of this article.

I shall refer to *intensity*, the physical term for amplitude of sound waves, rather than *loudness*, the psychological impression that varies directly with amplitude, because the experiments are based partly on measurements of intensity. On the other hand, I shall refer to *pitch*, the psychological term, rather than *fundamental frequency*, the physical term, for the sake of brevity.

DEFINITION. By *stressed syllable* I mean one that occurs IN AN UTTERANCE with the kind of prominence that listeners identify as "stress." The same distinction can be made for English that Charles A. Ferguson makes for Persian: "The syllable on which the stress falls when a given word is uttered in isolation is said to have 'inherent' or 'potential' stress, or simply the WORD STRESS. It must be noted that this concept of word stress is essentially morphological. A statement of the type 'The word *X* in Persian has (word) stress on the third syllable' means in effect that the word *X* has two alternants, one with stress on the third syllable, one with no stress at all."[10] *Stress* as I use it does not refer to potential, or word, stress, but to stress that is actually there, imposed within an utterance. And for the most part the stress that I shall use as an example is the most prominent one in each of the utterances tested.

EXPERIMENTS BY OTHER INVESTIGATORS. Though a number of competent investigators have expressed doubts about intensity,[11] the only recent published experimental study to my

[9] Pike (pp. 83, 96) gives first place to intensity, but allows that duration may be a factor. Trager-Smith (*Outline*, § 1.61) correlate stress with loudness, and Edith C. Trager (in *General Linguistics*, II [1956], 2) says unequivocally, "there is only one component that matters—loudness."

[10] "Word Stress in Persian," *Language*, XXXIII (1957), 124-125. See also Uriel Weinreich, "Stress and Word Structure in Yiddish," in *The Field of Yiddish: Studies in Language, Folklore, and Literature* (New York, 1954), especially § 3.1.

[11] Arnold says (pp. 440-441), "articulatory force is frequently a difficult and, sometimes, an impossible yardstick for the recognition of linguistic stress in English." He is not concerned with the syllable that carries the main stress, but, given that syllable, with predicting where other stresses will fall, which he does on rhythmic principles. His statement therefore is a denial of the importance of intensity not on the main stress, but on the stresses that Smith and Trager would mark with /ˋ/ and /ˆ/. They, of course, insist on loudness there also.

Using an approach similar to Arnold's, Wiktor Jassem, in "Stress in Modern English,"

knowledge that calls it into question is "The Linguistic Relevance of Intensity in Stress," by H. Mol and E. M. Uhlenbeck.[12] The authors adduce arguments from Franz Saran (1907), who pointed to the ear's well-known sensitivity to pitch and its relative unresponsiveness to intensity, but who failed to dissuade other phoneticians from their adherence to dynamic stress. Mol and

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Bulletin de la Société Polonaise de Linguistique, Fascicule XI, pp. 23-49, is even more positive: "A definition of stress in terms of absolute force (or loudness) is an obvious impossibility. That the absolute breath-force (or loudness) of what are generally termed 'stressed' syllables may, under certain circumstances, be much less than the absolute force (or loudness) of 'unstressed' syllables hardly requires elucidation" (p. 29), and he cites Jespersen's *Lehrbuch*, p. 116, on this point. For him "the 'tonal accent' [is] wrongly called 'stress'" (p. 38).

Both Arnold and Jassem agree with the position that I adopt, which is that there is a special kind of syllabic prominence due to pitch, which should not be confused with other phases of stress.

H. E. Palmer "regards stressed syllables as those which take 'nucleus tones' in contrast to all other syllables which are pronounced with equal or nearly equal force" (quoted by Newman, p. 178; see below). Palmer's nuclei (see his *English Intonation* [Cambridge, 1922]) are the nearest to the pitch accents that I describe in this article. The principal difference is that the nuclei are sentence stresses (p. 7), and while other syllabic prominences due to pitch are scored in his notation, he lumps them under "heads." For example,

what a remarkably pretty little house

contains, for him, a nuclear tone on *house*, and what precedes is merely a "broken scandent head" (p. 46). As set forth in § 7 of this article, I would put pitch accents on *what*, *-mark-*, and *pret-*, those on *-mark-* and *pret-* being the same as the "nuclear" in Palmer's (p. 39)

Could you spare me one?

on the word *spare*.

Stanley S. Newman, "On the Stress System of English," *Word*, II (1946), 171-178, holds to the intensity theory but with reservations: "Altho force of articulation is the primary medium thru which the stress phonemes are externalized, this phonetic feature is not the exclusive medium of stress" (p. 171). He distinguishes between "expressive accents," in which articulatory force is secondary to or at least equaled by pitch and quantity (p. 173), and "stress accents," where intensity predominates. My view differs in that a large part of what Newman puts under the stress accents I would put under the expressive accents.

E. A. Glikina, in "An Attempt at an Experimental Study of the Elements of Dynamic Stress (With Reference to English)" (in Russian), *Voprosy jazykoznanija*, 1958, no. 5, 18-85, showing no awareness of recent laboratory investigations in the United States, and employing no synthetic devices, found that in 250 pronunciations of compounds by native Englishmen, accentual prominence was generally achieved by a combination of intensity, duration and pitch, although any one factor could render the distinction when the others were equal.

[12] *Lingua*, V (1956), 205-213. The same authors adduce other theoretical arguments against intensity in a later article, *Lingua*, VI (1957), 346.

Uhlenbeck experimented with the *pérmit—permít* pair, altering intensities in such a way that the stresses could not be signaled by them, and finding that nevertheless the stresses were clearly heard. They are perhaps too categorical about the absolute irrelevance of intensity, but their conclusions are probably true as regards its relative importance: "It is obvious that the decoding-system of the ear does not use any amplitude information. The system is concerned with the recognition of shapes or forms." The prominences to which I have referred are, of course, shapes—configurations of pitches.

At about the same time, Dennis Fry was conducting experiments at Haskins Laboratories using similar sets of minimal pairs, to determine whether pitch or duration was the better cue to stress. He had previously used the sets in pitting intensity against duration, and found that duration on the whole is a better cue.[13] The later experiments showed pitch to be superior to duration,

[13] "Duration and Intensity as Physical Correlates of Linguistic Stress," *Journal of the Acoustical Society of America*, XXVII (1955), 765ff.; "Experiments in the Perception of Stress," *Language and Speech* I (1958), 126-152.

In a sense the separation of duration and intensity may be a false dichotomy, if what we are seeking is determinations of "loudness." It is possible that increasing only the duration of a syllable, and not its intensity, may cause at least some hearers to report it as "louder," owing to the integration of intensities over a period of time: where two otherwise identical syllables have the same intensity level but one lasts twice as long as the other, the longer syllable could then be said to have twice as much acoustic energy. Another way of putting it is that there is a point below which the failure to discriminate between changes of intensity and changes of duration is determined biologically (sensory discrimination) rather than culturally (function of naming). Apparently duration does operate in some such way, at threshold—a sound with a given intensity which would be altogether inaudible becomes audible when prolonged (see W. R. Garner and G. A. Miller, "The Masked Threshold of Pure Tones as a Function of Duration," *Journal of Experimental Psychology*, XXXVII [1947], 293-303). More recent evidence indicates, however, that above threshold, and at durations significant for judgments of syllabic stress, this may no longer hold. The experiments of Irwin Pollack ("Loudness of Periodically Interrupted White Noise," *Journal of the Acoustical Society of America*, XXX [1958], 181-185) suggest that for noises lasting longer than 1/10 second there is little change in loudness when duration is increased (provided the intervening "silences" are less than 1/3 second—a condition which is handily met in the normal stream of speech); and he refers to another study which sets the critical duration at around 1/20 second. A glance at Fig. 4 will show that durations of syllables normally regarded as stressable are well beyond the 1/20 to 1/10 second minimum, so that a further increase in duration would not create an impression of greater loudness, if these experiments have any bearing. I mention the point only because of the incidental observations in this article about duration and intensity, not because it affects the priority of pitch over intensity, or duration, or duration-intensity, however one may choose to look at the latter complex.

from which it is reasonable to infer that pitch is superior to intensity.

A more direct attack was made by J M. Cowan, in an experiment whose results have not yet been published but which he has been kind enough to explain in a personal letter. It consists, essentially, in producing speech from which all intensity-control has been cut out. A harmonic-rich tone is fed from an artificial larynx through a tube to the back wall of the pharynx, where the operator, articulating in a whisper, re-emits it. The pitch of the tone is continuously variable; besides the tone-control there is an on-off switch to contrast voicing and non-voicing, but there is no control for intensity. Listeners report very good "stress" contrasts.

TESTS 1, 2, AND 3: INTENSITY AND PITCH IN NATURAL SPEECH.

Test 1. The sentence *Wouldn't it be easier to wait?* was recorded by the experimenter in two patterns of pitch, as seen in Fig. 1. In one (solid line), the syllable *wait* was "obtruded" from the pitch line by giving it a rapid fall. In the other (dotted line), the syllable *ea-* was obtruded by putting the rapid fall immediately after it, while the syllable *wait* was "embedded" in the following gradual curve. (This pitch contrast is the same one reported by James Sledd for the sentence *It's utterly ridiculous*, opposing the syllables *ut-* and *-dic-*.[14]) Intensities (peak value on vowel of syllable) were uniform in the four syllables: *ea-=wait=ea-=wait.*

Eight listeners[15] were asked to say whether *easier* or *wait* contained the major stress. They had no difficulty, despite the lack of contrast in intensity: the vote in each instance was in favor of the similarly obtruded syllable.

Test 2. Smith and Trager[16] utilize an example that has been widely quoted: *The Pennsylvania Railroad is the main Pennsylvania railroad.* Without giving further details, it is enough to say (1) that *Pennsylvania Railroad* and *Pennsylvania railroad* are supposed to be distinguished by the arrangement of dynamic stresses (loudnesses), and (2) that in both instances in normal speech the principal stress on *railroad* is supposed to be louder than that on *Pennsylvania.*

[14] In *Litera*, III (1956), 38.

[15] Except as otherwise noted, listeners in all experiments were researchers and technicians at Haskins Laboratories.

[16] § 4.3.

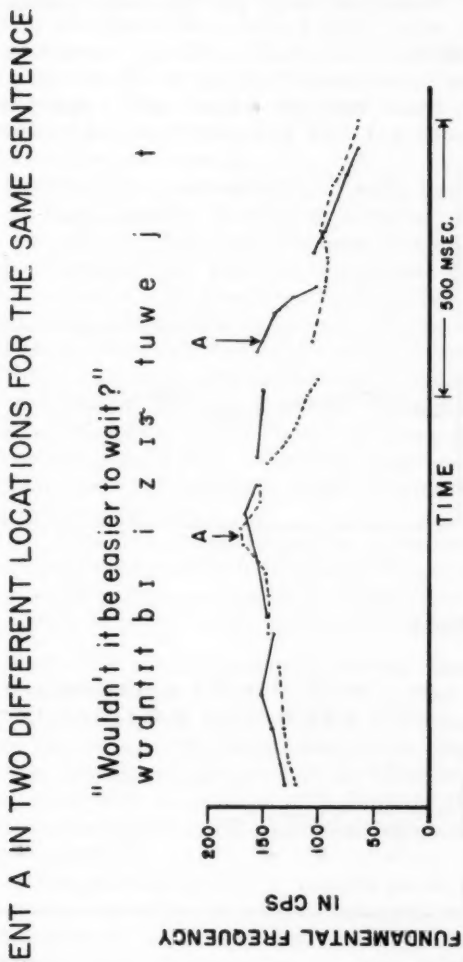
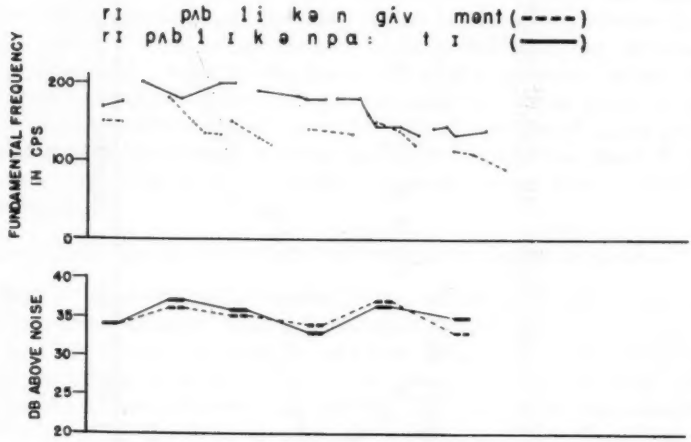

ACCENT A IN TWO DIFFERENT LOCATIONS FOR THE SAME SENTENCE
"Wouldn't it be easier to wait?"
wʊdnɪɪt bɪ i z ɪɜ tʊwe j t
A
A
FUNDAMENTAL FREQUENCY IN CPS
200
150
100
50
0
TIME
500 MSEC.

Fig. 1

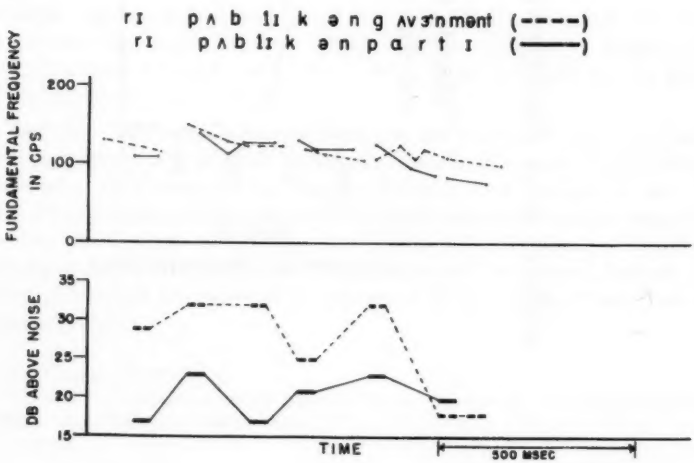
Fig. 2

"Republican Party" vs "Republican government"

SPEAKER A



SPEAKER B



As a less complicated example, since *Pennsylvania* contains what is usually marked as a secondary stress, I substituted the word *republican* in two contexts, which I infer to be analogous to those of *Pennsylvania:* (1) *He's a Methodist and belongs to the Republican Party* and (2) *It's too bad that Spain doesn't have a more republican government.* This test is reported more fully elsewhere,[17] but I reproduce the tracings in Fig. 2 to show a visual display of pitches in natural speech.

Six speakers recorded the two sentences. Only two could be judged by ear to have made a distinction between *Republican* and *republican*; the others apparently inferred a contrast with *Democratic.* Spectrograms were made of the phrases *Republican Party* and *republican government* as uttered by these two speakers. The pitch and intensity information appears in Fig. 2.

As the curves show, there are two kinds of pitch prominence. In *Republican Party* (solid line), the syllable *-pub-* stands out by reason of being skipped up to; after that the line remains fairly uniform until the syllable *Par-*, where there is a sharp drop. In *republican government* (dotted line), however, *-pub-* is obtruded both by an upskip and by following rapid downmotion, and *gov-* is obtruded in the same way.

The information from the intensity profile is irrelevant, and in one instance actually runs counter to the Smith-Trager markings: Speaker A gives slightly more intensity to *-pub-* than to *Par-*. His recording gave no impression of a contrastive stress.

Test 3. In a suggestive article comparing German and English intonation patterns,[18] Hugo Mueller observes that English numeral-plus-noun combinations tend to place the numeral on a higher pitch than the noun. He follows the Smith-Trager pitch-and-stress markings, giving an example such as ²*That took* ³*lwênty minutes*¹ #, and interprets as follows: "In English, *the number* tends to have the highest pitch in the phrase, although it does not bear the strongest stress."

I judge this description to be correct as regards pitch. I would generalize it, however, to refer to all QUANTIFYING MODIFIERS, i.e., modifiers that show an amount or degree of something rather than a characteristic that distinguishes it from other things. I would expect the word *single* in *I couldn't contribute because I*

[17] in my article "On Certain Functions of Accents A and B," *Litera*, IV (1957), pp. 80-89.

[18] "Some German Intonation Patterns and Their Relation to Stress," *Modern Language Journal*, XL (1956), 28-30.

was flat broke; I didn't have a single dollar to show the same trait as *twenty* in Mueller's example, and to contrast with the word *single* in a context where it is differentiating, e.g. *I'd have been glad to contribute if I'd had a single dollar, but all I had was some small change and a five-spot.*

This contrast with *single* was put to seven speakers, and the majority confirmed the predicted arrangements of pitch as judged by ear. An analogous contrast was made use of for more precise measurement: the word *solid* in the sentences *I waited a solid hour on that cold street corner* and [*How come it doesn't bend?*] *Because it has such a solid frame.* As this experiment is also reported in detail elsewhere,[19] I shall limit myself to the results that bear on the question of intensity:

Of six speakers, three produced both utterances as predicted (all six did *solid hour* as predicted). Pitch and intensity were measured for these three. Peak intensities for the three syllables of *solid hour* were as follows, in decibels above noise: 27-30-31; 34-32-35; 35-30-32. In only one of the three is *hour*, supposed to be the "strongest" stress, different by more than 1 db from both of the other syllables—this is the third example—and there it is 3 db LESS than *sol-*. It is obvious that the hearer is getting no information from intensity here.

TESTS 4, 5, 6, AND 7: INTENSITY AND PITCH IN SYNTHETIC SPEECH.

The Voback is a machine developed at Haskins Laboratories[20] enabling the experimenter to apply varying pitches or intensities to an artificial spectrogram by means of a hand-drawn pattern. For experimental purposes it has several advantages over natural speech; it is more flexible, for example, in that the pitch can be controlled within a fraction of a cycle per second, and in that a synthetic utterance can be stopped and held at any point to read off the precise pitch or to listen to any characteristic of the painted pattern. Spectrograms of natural speech will not yield precise information about pitch in jumps smaller than about 4 cps, and the effect of stopping a tape recorder is, of course, silence. The recordings used in the tests about to be described were made by the Voback.

[19] "On Certain Functions of Accents A and B," pp. 81-83.

[20] See John M. Borst and Franklin S. Cooper, "Speech Research Devices Based on a Channel Vocoder," *Journal of the Acoustical Society of America*, XXIX (1957), 777.

Test 4. The synthetic sentence *Break both apart* was given various treatments of intensity and pitch, designed to throw stress on one or another element. Seven listeners were asked simply to indicate the syllable or syllables that they heard as stressed. Here are some of the stimuli and their results:

1. Syllables *break* and *-part* given an inflected pitch of 120 cps dropping to 100 (about 3 semitones), other syllables held at 100. Syllable *both* 10.5 db more intense than *break* (at least "twice as loud"), and 7.5 db more intense than *-part*. Majority hear stress on *-part*.

2. Syllable *break* inflected, 120-100 cps, other syllables at 100 cps monotone. *Both* 11 db more intense than *break*, 9 db more intense than *-part*. Majority hear stress on *both*. A massive increase in intensity overcomes a comparatively small inflection of pitch, in this particular pitch pattern.

3. Syllable *break* at 120 cps, rest at 100 cps. *Both* 2 db more intense than *break* and 1 db more intense than *-part*. A large majority hear stress on *break*. The smaller difference of intensity is overwhelmed by the difference of pitch.

4. All syllables at 80 cps monotone. *Break* 10.5 db more intense than *both* and *-part*. A small majority hear stress on *break*. The massive increase of intensity here does not do as well as the comparatively small rise of pitch in (3), in putting the stress on *break*.

5. Syllable *both* given an inflected pitch of 90-80 cps, the rest at 80 cps monotone. Syllable *both* 6 db LESS intense than *break* and *-part*, but heard as stressed by the majority.

Other patterns and treatments could be listed, but these are typical, and indicate the ease with which changes in pitch register as stress and the difficulty that changes of intensity have in competing with them.

Test 5 is designed to show the relative power of certain changes of pitch and intensity. The synthetic sentence *Many are taught to breathe through the nose* was played at a monotone, and without manipulations of its intensities, to six listeners, to find how they considered it to be stressed. In 7 out of 12 judgments they favored *breathe* (3 *many*, and 1 each *nose* and *taught*). Knowing that the listeners already favored *breathe*, the experimenter now set about increasing the preference by adding intensity to this word, making

it 3 to 7 db more intense than *taught* or *nose*.[21] The object was to see how it would fare when changes of pitch were applied to these two other words.

The word *taught* was given 12 simple rises of pitch in 5 cps steps from 95 to 150 cps, the rest of the sentence being kept at a 90 cps monotone. The same treatment was given to *nose*, and in addition, the entire series was done again on *nose* with an inflected rise instead of a simple rise, the first half of the syllable being at the higher pitch and the second half at the reference pitch. Responses of the six listeners to the stimuli are shown in Fig. 3.[22]

A pitch rise of from 5 cps (less than a diatonic semitone) to 15 cps (less than a minor third) is sufficient to overcome both the initial bias in favor of *breathe* and its added intensity.

Test 6. The Smith-Trager system allows for rises in pitch associated with stress, but considers them "allophones of the pitch phonemes."[23] One to whom this point of view has become second nature is likely to say, "Of course if pitch rises when intensity rises, the hearer who is given a stimulus in which there is a change in pitch automatically interprets it as a change in intensity; the pitch is only an indirect cue to the stress, but since it is the only cue present, it becomes effective." That is to say, a feedback is assumed from pitch, through intensity, to stress. While this argument hardly refutes the experiments where both pitch cues and intensity cues are present, it needs to be tested for whatever other plausibility it may have. This can be accomplished by asking the question in reverse: "When there are changes in intensity, can they be heard as changes in pitch?" If the answer is yes, this means that the gun shoots both ways.

The synthetic sentences *Break both apart* and *But would many return?* were given various treatments of pitch and intensity, including some in which the utterance was held at a monotone but one syllable was given an increase of intensity, and others in which the same utterance had both pitch and intensity changes.

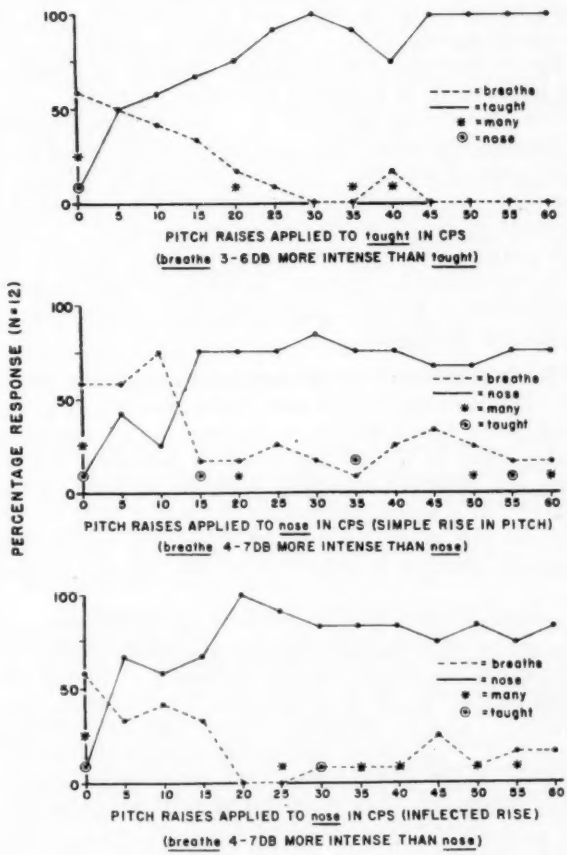
[21] The 3-7 fluctuation was not intended, but resulted from an inherent characteristic of the Voback.

[22] It should not be necessary to point out that normal precautions were taken in administering this and other tests. The stimuli were randomized and four other stimuli, two with pitch changes on *many*, were introduced to divert attention from the systematic changes lest the hearers assume that *breathe*, *taught*, and *nose* were the only words that could contain the major stress.

[23] P. 43.

Fig. 3

VARIABLE PITCH VS CONSTANT INTENSITY-DURATION AS A CUE TO LOCATION OF SENTENCE STRESS



ma	ny	are	taught	to	breathe	through	the	nose
182	238	168	182	126	112	70 476	196	182	84	518

DURATION IN MSEC.
(CONSTANT FOR ALL TEST STIMULI)

The listeners were asked to indicate the syllables that went higher or lower in pitch than the reference level.

Responses showed a tendency to hear a marked rise in intensity as a rise in pitch, when the stimulus was actually a monotone. This tells us that it is just as possible to mistake a change in intensity for a change in pitch as it is to mistake a change in pitch for a change in intensity. The feedback works both ways.

On the other hand, the responses showed no tendency to hear even a large DROP in intensity as a drop in pitch. Instead, the subjective impression was that of some external interference, as if the speaker had turned his face away or the wind had suddenly snatched part of a word. Intensity seems to be significant only when it rises—as we would expect if it were a voice qualifier rather than something linguistic in the narrower sense. Configurations of pitch work in either direction—up or down; in fact, some of the listeners responded to changes in pitch without knowing which way they went. Pitch is again the more reliable cue, in respect of this greater flexibility.

Test 7. Assuming now what seems to have been amply demonstrated—that intensity is at best UNNECESSARY as a cue to stress and that pitch alone will serve so long as an utterance is kept reasonably close to the normal range of intensity and duration—one may ask whether intensity perhaps at least contributes to the QUALITY of a stress, to making it "sound right." To inform ourselves on this point, we devised a test the results of which are published in another journal[24] and will merely be summarized here.

The synthetic sentence *Alexander's an intelligent conversationalist* was given pitch-marked stresses at points where the intensity was BELOW the maximum for the utterance. Boosts of intensity were then applied at those points, and the resulting stimuli, with and without added intensity, were judged for quality by 62 listeners.[25] Minimum intensities turned out to be slightly preferred to small additions of intensity; small additions were somewhat preferred over large additions of intensity; and the greatest preference was shown for no addition of intensity as against large additions.

[24] Bolinger, "On Intensity as a Qualitative Improvement of Pitch Accent," *Lingua*, VII (1958), 175-182.

[25] A pre-test by Laboratory personnel plus a large-scale test by naive listeners at the University of Connecticut.

TESTS 8 AND 9: INTENSITY OPPOSED TO OTHER CUES THAN PITCH.

Listeners might well feel that in sequences like *Pennsylvania Railroad*, *republican government*, etc., the stress at the end gives the subjective impression of greater prominence than the earlier stress or stresses. With this I would agree. As we have seen, however, measurements of intensity do not confirm it. Neither do the measurements of pitch in *republican government*, where the same kind of prominence (upskip followed by rapid downmotion) is present in both, but the second, instead of being more marked than the first, is less marked.

The only remaining measurable factor that is usually recognized to play a role in stress is duration. But a fourth possibility suggests itself: position. It is conceivable that stress is climactic, and that we attribute extra intensity to the position at the end, even when it lacks it phonetically. This is suggested by our tendency to shift the sentence stress toward the end at the price, sometimes, of distorting the word stress as a result.[26] If position overrides pitch, which in turn overrides intensity, we have one explanation of why the end stresses are so consistently marked as "louder."

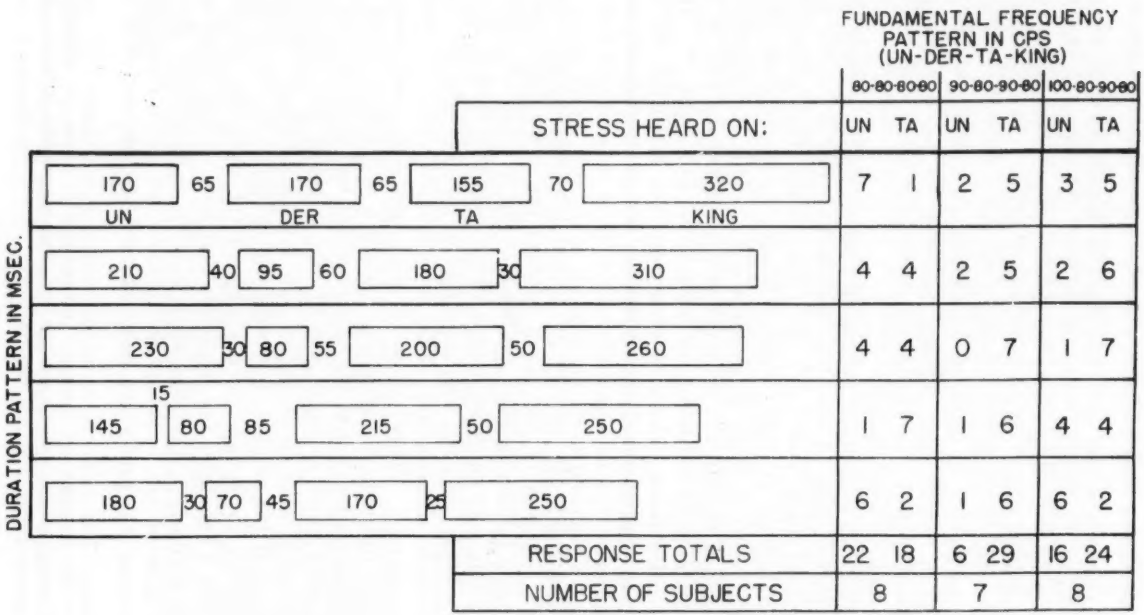
Test 8 pits the cues of pitch and position against each other. Five patterns of the word *undertaking* were hand-drawn with varying syllable lengths, as shown in Fig. 4. Various combinations of pitch were superimposed, and the resulting stimuli were judged by several listeners, who were told to indicate whether they heard the word *úndertaking*, 'what a mortician does,' or *undertáking*, 'enterprise.' There were 16 pitch patterns comprising a total of 615 individual responses. In all but 3 patterns the majority of listeners reacted as the experimenter had predicted on the basis of pitch, and in only one of the 3 could the discrepancy be correlated with duration (i.e., the fault lay with a wrong interpretation of the pitches, not with the influence of duration). This confirms Fry's experiments where pitch overrides duration.

Fig. 4 tabulates the responses that bear on the problem of pitch and position. The first pitch pattern, a monotone, shows a bias in favor of *ún-*. When *un-* and *-ta-* are given equal pitch rises, however, the preference shifts radically to *-tá-*, and even when *un-*

[26] For examples, see Bolinger, "Intersections of Stress and Intonation," *Word*, XI (1955), 199-201, and "English Stress: the Interpenetration of Strata," § 6, in *Study of Sounds* (Tokyo, 1957), pp. 295-315.

Fig. 4

STRESS JUDGMENTS FOR SEVERAL VERSIONS OF THE WORD "UNDERTAKING"



	FUNDAMENTAL FREQUENCY PATTERN IN CPS (UN-DER-TA-KING)					
	80-80-80-80		90-80-90-80		100-80-90-80	
STRESS HEARD ON:	UN	TA	UN	TA	UN	TA
170 65 170 65 155 70 320	7	1	2	5	3	5
210 40 95 60 180 30 310	4	4	2	5	2	6
230 30 80 55 200 50 260	4	4	0	7	1	7
145 15 80 85 215 50 250	1	7	1	6	4	4
180 30 70 45 170 25 250	6	2	1	6	6	2
RESPONSE TOTALS	22	18	6	29	16	24
NUMBER OF SUBJECTS	8		7		8	

is made twice as prominent as *-la-*, *-lá-* is still favored by the majority. This confirms the tendency to hear the latter of two pitch-marked stresses as "louder."

Test 9. In the Smith-Trager system, constructs such as *light-house-keeper* and *light housekeeper* ('housekeeper who is light rather than heavy') are supposed to be distinguished by different loudnesses at different points. As the naive speaker is likely to think that the difference depends more on degrees of separation than on loudness, an experiment was performed to oppose the two cues of disjuncture (separation of syllable centers) and intensity. Here again it developed that the information from intensity was irrelevant, and that the difference was in fact signaled by separation.[27]

With all the phonetic evidence pointing away from intensity as the thing that matters most in utterance stress, how can we account for its hold on the imagination? The answer lies, I think, in our folklore, and reflects not the acoustic signal but its linguistic function. The parts of our utterance that we stress most noticeably are the ones about which we want to be most forceful, and the kinetics of that force is felt and seen in a number of ways. A writer underlining the important parts of a message does it energetically. A speaker bobs his head and swings his arms in time with his stresses.[28] With this pugilistic obligato to the linguistic tune, it is hardly surprising that the tune became identified with the exercise that went with it; hence force, intensity, loudness. Finally, the pitch direction most fundamental to stress is up, and rising pitch bears a "pre-linguistic," physiological relation to physical tension.

Having given up the more usual definition of stress, I think it is wise, because of associations, to give up the term also. From this point on I shall therefore refer not to stress but to PITCH ACCENT, or simply ACCENT, meaning prominence due to the configuration of pitches.

4. Pitch Prominence in General

The experiments relegating intensity to a secondary position have shown that it is unnecessary to take great precautions about

[27] See Bolinger and Louis J. Gerstman, "Disjuncture as a Cue to Constructs," *Word*, XIII (1957), 246-255.

[28] A. J. Vanvik, observing the orators at Hyde Park, noted that how they stressed a word such as *salvation* could be determined by their gestures without actually hearing the syllabic contrasts. See *Maître Phonétique*, No. 103, Jan.-June 1955, p. 8.

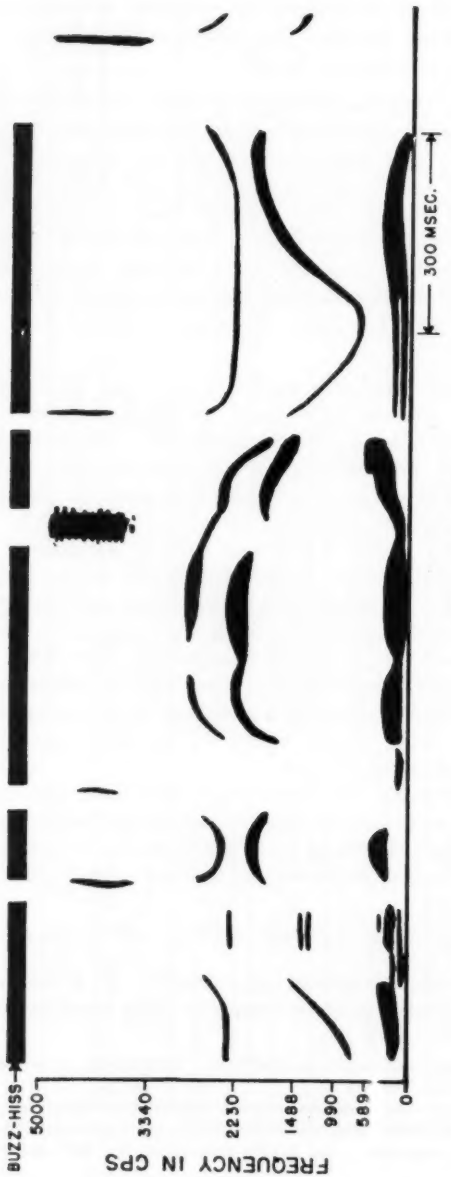

BUZZ-HISS
5000
3340
2230
1488
990
589
0
FREQUENCY IN CPS
300 MSEC.
TIME IN MSEC.

Fig. 5

small random differences in intensity between one syllable and another in a synthetic utterance. Since the Voback does not deliver perfectly uniform intensities throughout the pitch range (nor does the human voice, though it is somewhat more stable in this regard), without these proofs it might have seemed necessary to specify intensities at every point. As it is, we may attend simply to pitch markings and let the minor fluctuations of intensity fall where they may.

Test 10 uses the same sentence, *Wouldn't it be easier to wait?*, as Test 1, but synthetic. The hand-drawn spectrogram is shown in Fig. 5. (This is done with white paint on transparent film. The pitch information, none of which appears here, is painted in a solid design at the top, with its upper edge tracing the rises and falls. The broken block lines directly beneath are for buzz (voice) and hiss (whisper). The rest of the painting controls the segmental phonemes—formants, transitions, bursts, releases, and frictions of the vowels and consonants.) Some 38 patterns of pitch were overlaid and judged by 9 listeners, who were told to indicate whether the principal accent ("stress" was the term used in the instructions) fell on *easier* or on *wait*. The responses indicated:

1. That when only one item is given pitch prominence, it is heard as accented. The pitch movement may be UP TO, DOWN TO, or DOWN FROM the accented syllable. Examples (the frequencies hold for all following syllables up to the next indicated frequency):

PATTERN	DIRECTION	VOTE FAVORING
100 *Wouldn't it be easier to* 120 *wait?*	up to	9-0, *wait*
100 *Wouldn't it be* 120 *easier to wait?*	— —	9-0, *easier*
100 *Wouldn't it be easier* 120 *to* 100 *wait?*	down to	9-0, *wait*
100 *Wouldn't it* 120 *be* 100 *easier to wait?*	— —	8-1, *easier*
100 *Wouldn't it* 120 *be ea*100*sier to wait?*	down from	9-0, *easier*
130 *Wouldn't it be ea*110-100*sier* 100 *to wait?*	— —	9-0, *easier*

Or the movement may be within the syllable:

100 *Wouldn't it be easier to* 100-130 *wait?*	up in	8-1, *wait*

2. That when one of the items is involved in some movement of pitch, but not enough to make it stand out from its environment, the other, if more clearly marked, is heard as accented. For example:

a) Movement down to the accented syllable, other syllable embedded in gradual upmotion:

80 *Would*85*n't* 90 *it* 95 *be* 100 *ea*105*si*110*er* 115 *to* 80 *wait?* 6-3, *wait*

80 *Would*85*n't* 90 *it* 95 *be* 80 *easier to wait?* 8-1, *easier*

b) Movement down from the accented syllable, other syllable embedded in more gradual downmotion:

130 *Wouldn't it be ea*110-100*sier* 98-92 *to* 90-70 *wait ?* 7-2, *easier*

130 *Wouldn't it be ea*130-120*sier* 118-108 *to* 100-70 *wait ?* 7-2, *wait*

These last two patterns reproduce the crux of Test 1—note the accent-forming 30 cps drop across *wait* in the second pattern as against the non-accent-forming 20 cps drop in the first.

c) Movement down from the accented syllable, with an undulation on the other syllable:

130 *Wouldn't it be ea*110-100*sier* 96-86 *to* 94-70 *wait?* 9-0, *easier*

3. That when two similar accents are obtruded with equal clarity, the second is more prominent (confirming the remark above about position). Example of two accents both skipped down to:

130 *Wouldn't it be* 115 *easier to* 100 *wait?* 8-1, *wait*

5. Predictable Ambiguity.

If prominences of the kind that I have described are the true basis of the accents, it should be possible to predict when a given utterance may be taken in more than one way. For example, in a configuration of the shape ⌐_ there are two "corners," either of which may be the significant one. If the top corner, we describe it as an accent that is "skipped down from." If the bottom corner, it is an accent that is "skipped down to." The fact that the linguistic signal is mixed does not refute it, but

simply means that under the circumstances the hearer looks to another cue. Take the utterance

I
broke it

in which both *I* and *broke* occupy corners. In the context where A says to B,

ny!... catch
John you're going to it for breaking this vase!

and C, a third party, confesses *I broke it*, it is *I* that is accented. But after

What happened to this vase?

the same *I broke it* is a disgusted reply accenting *broke*. The cue is contextual redundancy. In place of or along with the contextual cue, either *I* or *broke* may be phonetically degraded (though neither has to be), and when it is, the accent is thrown to the other word. In this sense, the allegro and lento forms of words perform an accentual function. Normally an accented syllable must not be phonetically degraded.

The commonest supporting cue is flanking by unaccentable syllables. The contrast between the following two utterances is clear:

It's the man who broke it

It's the man who broke it

The unaccentable syllable *who* helps to embed *man* in the first instance and *broke* in the second, giving *broke* the accent in the former and *man* in the latter. How we know that *who* is unaccentable will be discussed in the next section.

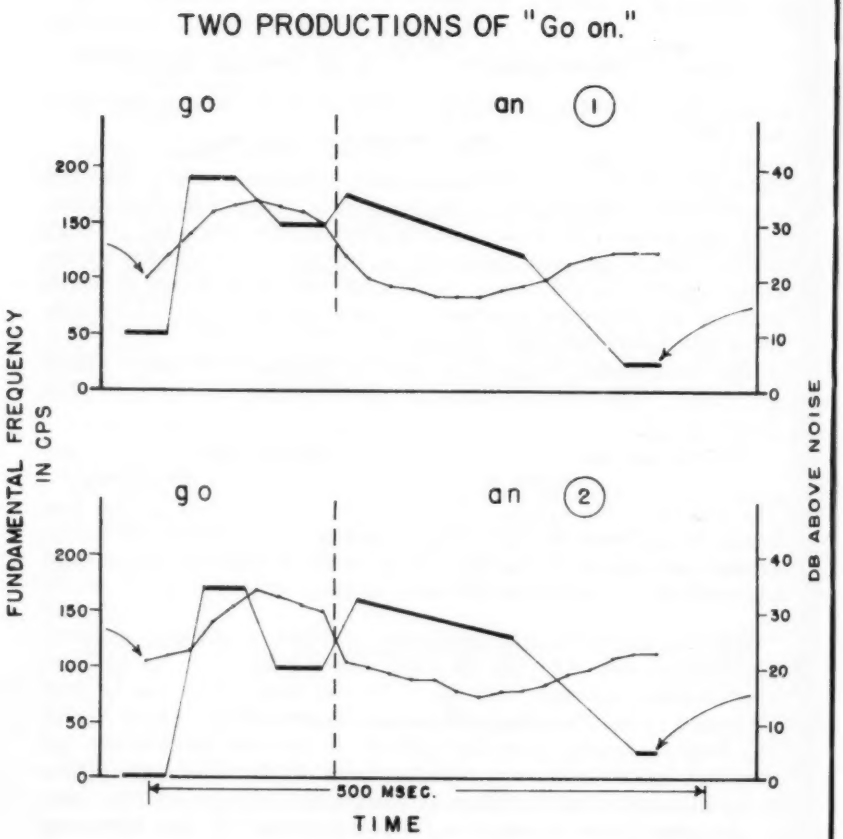
Test 11 embodies an ambiguity, contrived without phonetic degradation and with no flanking unaccentables, so as to force identification through context. It was decided to use a short command of the *Run along, Wait here, Come in* type, where American English readily puts the adverb at the low pitch when the speaker's intent is to coax, and to match it with the same phrase in a different context where the accent is clearly on the verb.

The experimenter made a tape recording of the following: *You say you want us to find your missing husband, but you certainly haven't given us much to go on, go on.* The two instances of *go on* were given the shape

Go
on

and made as nearly identical as possible. The pitch and intensity profiles appear in Fig. 6.

Fig. 6


TWO PRODUCTIONS OF "Go on."
go
an
1
2
200
150
100
50
0
40
30
20
10
0
FUNDAMENTAL FREQUENCY IN CPS
DB ABOVE NOISE
500 MSEC.
TIME

Copies were made of the recording. In one copy. the second *go on* was cut out, and the remainder constituted Stimulus II. In the other copy, everything was eliminated except *Go on, go on.* A second voice now recorded a long passage which was interrupted in the middle, at which point the *Go on, go on* was inserted, whereupon the second voice resumed. The effect was that of someone reading, stopping before reaching the end, being urged by another person to continue, and finally completing what was begun. This interchange constituted Stimulus I.

The listeners were asked to indicate which word carried the accent (instructions were explicit, using the terms *stress*, *accent*, and *emphasis*, to avoid a possible tendency to judge phonetically without regard to context), and also to say whether what they heard sounded like normal English. Three groups of varying degrees of sophistication participated: A, Laboratory staff and researchers; B, a class in Spanish phonology; C, a sophomore-level college class. Responses are shown in Table 1.

TABLE 1. JUDGMENTS OF ACCENT AND ACCEPTABILITY IN TWO CONTEXTS OF *go on*. (In Group B the same two marked "not normal" as marked *ón*; in Group C, however, none of those who marked "not normal" were among those who were judging 'continue' as *go ón*. There is therefore no correlation here.)

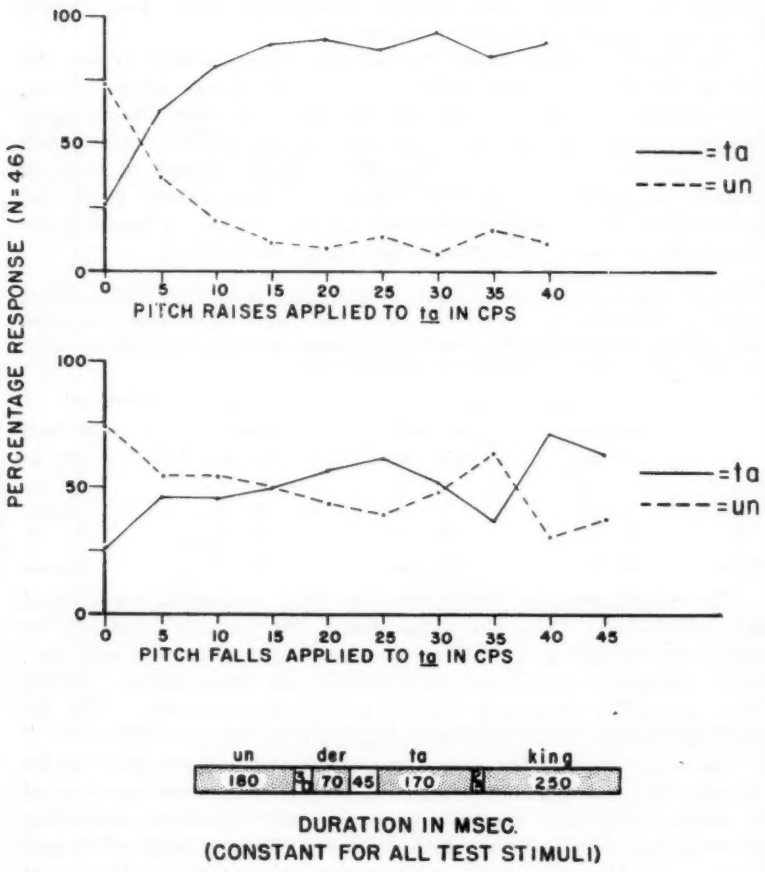
Group	Stimulus I 'continue'				Stimulus II 'rely on'			
	Accent on		Normal?		Accent on		Normal?	
	go	*on*	yes	no	*go*	*on*	yes	no
A	5	4	9	0	8	1	9	0
B	4	2	9	2	6	0	6	0
C	19	5	16	8	20	4	18	6
Totals	28	11	29	10	34	5	33	6

The experimenter's judgment that both instances are normal was confirmed. On the other hand, the greater tendency to locate the accent as *go ón* for 'continue' and *gó on* for 'rely on', while probably significant, is certainly not impressive. In all three groups the majority marked *gó* in both instances. Yet the analyst "knows" that 'continue' has to be *go ón*.

The reluctance of the listeners to hear the bottom pitch as an intentional prominence confirms the difficulty that we experienced in other tests, and suggests a reason why speakers—including phoneticians—instinctively look for correlations of HIGH pitch and stress. In the other tests, a downward obtrusion nearly always gave data which while reliable were not as clean as those obtained with an upward obtrusion.

Fig. 7

VARIABLE PITCH VS CONSTANT INTENSITY - DURATION AS A CUE TO LOCATION OF SYLLABLE STRESS



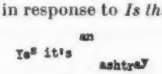
Test 12 shows this graphically. From the hand-drawn patterns of *undertaking* in Test 8, the one was chosen which had shown the clearest bias in favor of *un-* (the last pattern in Fig. 4). This pattern was then given pitch treatments similar to those in Test 5, with successive 5 cps rises on the syllable *-ta-*. In addition to the upward obtrusion, downward obtrusions also in 5 cps steps were put on the same syllable. The resulting stimuli, randomized and mixed with other stimuli, were played to 46 naive listeners at the University of Connecticut. The data are schematized in Fig. 7. The top diagram, representing the upward obtrusions, is similar to those of Fig. 3. The bottom diagram, representing the downward obtrusions, leaves little doubt that a prominence in that direction is effective, but also makes it clear that upward obtrusions are superior to downward ones.

The hypothesis that I advance to account for at least part of the difference in effectiveness between the two directions is that when we ask listeners to point to a "stress," and then give them a downward obtrusion, we miscue them. Downward obtrusions give perfectly normal syllabic prominences, as the judgments of Test 11 prove. But "stress" popularly means 'being emphatic about something.' The downward obtrusion seems to contradict this. Its use in situations where gentleness or restraint is called for is widely recognized.[29] Listeners are therefore willing to

[29] Pike (*Intonation*, § 4.3.1) gives 'encouragement' as the meaning of

I have pointed out the frequency of the low pitch in questions addressed to a stranger, in "The Intonation of Accosting Questions," *English Studies*, XXIX (1948), 109-144. In Test 7 (text, above), listeners were apparently more reluctant to approve of added intensity on the bottom pitch than on higher pitches, suggesting a preference for "softness" there. Jones notes (§ 1069) that *All right* is usually

and that if the directions are reversed one "may have the effect of a threat." Fred W. Householder, Jr., calls my attention to an example by Martin Joos involving a full answer to a yes-no question: in response to *Is this an ashtray?* it would probably be



He suggests the meaning 'de-emphasis or restraint.' Palmer and Jassem, however, take their metaphors literally. Palmer considers

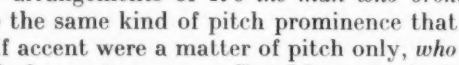
accept the downward syllabic prominence, but not always to interpret it as something "stressed."

What emerges from this is that while prominence as such gives the accent, accents are of more than one kind. It is best then to speak of PITCH ACCENTS, in the plural, and to look for kinships and differences. The search will also illumine that other part of the accentual complex, the unaccentable syllable.

6. Favored Accents and the Learning of Accentual Structure

Of the two arrangements of *It's the man who broke it* (p. 18), one gives *who* the same kind of pitch prominence that *man* has in the other. If accent were a matter of pitch only, *who* would have the same kind of accent as *man*. But I have used *who* as a means of warding off an accent, not of itself acquiring one, and referred to it as "unaccentable."[30] There is an apparent contradiction.

The explanation probably lies in the manner in which the lexicon is learned. A child confronted with the two patterns



would have no cue, from pitch, to tell whether the accent is on

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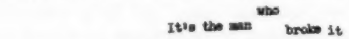
to have "a protesting or exclamatory character" (*English Intonation*, p. 80). Jassem regards

as "stupefaction, surprise combined with either incredulity or disapproval" (*Intonation of Conversational English* [Wrocław, 1952], p. 29). The verbal context does suggest this, of course, but the speakers are probably restraining themselves. The words and pitches pulling against each other are reminiscent of familiar paradoxes of literature, "an icy smile," "a cheerless laugh." For Maria Schubiger, *English Intonation: Its Form and Function* (Tübingen, 1958), the lowered pitches are "regardful."

[30] Actually this is not quite true. *Who* may be accented as a means of affirmation. Normally the configuration would differ slightly from the way it was set forth in § 5 above: specifically, the *who* would rise in pitch above what precedes:



'He is the very person who did it,' analogous to the rise in pitch on other function words for the same purpose of affirmation, commonest in the auxiliary verbs, e. g.

but also found on other words:

Note the example *in Rumania*, text § 8. But these instances are too infrequent to matter in the stigmatizing of most function words as unaccentables.

dai-, on *-dʒɛs-*, or on *-lə-*, assuming that he has learned to respond to pitch prominence in a general way. This does not bother the adult, who KNOWS that only *-dʒɛs-* can receive the pitch accent, and is accordingly able to respond to and produce more than one KIND of pitch accent using the same phonetic data—high pitch, fall, and rise—differing only in the location of the accentable syllable on the configuration.

The question is, how does the adult know that only the one syllable is accentable, since pitch contrasts like those of the two examples did not give him this information as a learner? The answer is that it is a matter of frequency. About 70 percent of the time, the accented syllable is what might be called a cliff-hanger—it is obtruded by means of a subsequent fall in pitch.[31] Most of these—virtually all of them when they are citation forms and a large majority under other conditions of special emphasis (conditions under which learning is most likely to take place)—are skipped up to as well. With sufficient normal contexts of the type

Cu ges
cumbers are indi tible

it does not matter that the pitch relationships are occasionally reversed, for the syllables *cu-* and *-ges-* will have been learned quite early as the potentials for pitch accent in their respective words. Thereafter other obtrusions, with different meanings, become possible.

The reverse of the coin is the almost unbroken regularity with which certain segments are NOT obtruded, They are learned as "generally unaccented," and this expectation enables them to be used in locations of great pitch prominence without being understood as accented; for instance the stopgap subject *there* in

There
wasn't any trouble

Similarly, certain syllable types are stigmatized as unaccentable. Typically these include the syllabic consonants and shwa, in words such as *dictum*, *turtle*, *warden*, *satchel*, and *forward*. A different syllable type flanked by these is in no danger of being misinterpreted, whatever the pitch direction.

Such is the built-in repertory of the language, on top of which

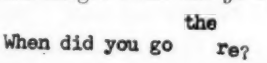
[31] I derive this percentage from Pike, *Intonation*, whose examination of 804 contours (p. 159) in three sample texts (p. 150) yielded 559 in which the syllable marked with stress was dropped away from (pp. 157-158).

the accents are laid. The English speaker's knowledge of the individual morphs and of morph and syllable types of his language gives permanent cues that serve as a foil to the phonetic nonce cues of pitch.[32]

Looking a little farther, however, we find that pitch accent depends also on certain other phonetic cues, nonce like itself, of which one is especially important: duration. From the synchronic standpoint, duration is to a large extent a co-variable with accent. From the diachronic standpoint, duration accounts in part for how the unaccentables got that way.

A pitch obtrusion requires time for its execution. When the pitch accent is embraced completely by a single syllable, the syllable is lengthened to accommodate the necessary range of pitches; so *go* is probably longer in the first than in the second of the following two examples:

And the second is in turn longer than the *go* of



in which *go* is embedded in what precedes the accent.

The experiments have made it clear that in the duration-pitch complex it is pitch that primarily signals accent. I therefore assume that duration is ancillary. Figuratively speaking, it is there IN ORDER TO make room for the accent (though I would not say that duration has no other function). This is supported by the following considerations:

1. Accented syllables are normally longer than unaccented ones in comparable positions within the utterance (i.e., in reference to junctures). (Cf. Jones, *Outline*, § 870.)

2. A long syllable—and here I follow Jones in assigning to length and concomitant vowel quality rather than to intensity the prominence of the "medial stress" in words like *asphalt*, *teapot*,

[32] The fact that the speaker must know the morphs disposes of the suggestion, often made, that nonsense syllables be used to test these patterns. The suggestion is a plausible one, for the worst obstacle to agreement about pitch stimuli is that there are always syntactic, morphological, and other cues present, and untrained listeners are apt to go off on a tangent, while trained listeners are apt to interpret them in terms of their training. Tests in which everything is stripped off except pitch would seem to be an answer; but, as I have tried to show, they will not work with the pitch accents.

hiccup (*Outline* § 920, footnote)—other than the one carrying the potential for pitch accent tends to be unstable. This is demonstrated by the history of loanwords in which the potential for pitch accent shifts its position. The nouns *hurricane*, *program*, *crayon*, *address*, *rupee*, *caravan*, and *mascot* were imported with the potential on the final syllable. They accommodated themselves to the normal accentual pattern of English nouns by shifting the potential forward. The syllable originally having the potential, however, did not lose its length immediately; for some speakers it is still long; for others it is short, with correspondingly centralized vowel (the *ACD* recognizes both stages for *máscot*, the earlier uncentralized [ɒ] and the later centralized shwa); for others it wavers. *Turquoise* has shifted without losing length, perhaps because of its syllable structure. *Intrigue* and *Portuguese* are shifting for some speakers, and have not had time to lose their length (which is furthermore supported rhythmically in *Portuguese*). This "medial stress" in many other words is uncertain: *cántŏn*, *cóncĕrt*, *infidĕl*, *récŏrd*, *cónvĕnt*, *lăbóo*, *áncĕstor*.

3. A short syllable to which the potential is shifted becomes long. I do not have measurements of this, but the evidence of vowel-decentralization seems clear enough: *pecan*, from [pi-] in *pecán* to [pi:-] in *pécàn*, in the dialects that have the latter; verb *survéy* [sɜ^{r}ˈveɪ] giving noun *súrvèy* [ˈsɜ^{r}veɪ], etc. Verb-based nouns such as *cóme-òn*, *cómbine*, *ímpòrt*, *pérvèrt*, *díscàrd*, and *tránsfèr* (or *tránsfer*) are typical. A favorite among linguists is the verb *to segmént* (or *ségmènt*), from the noun *ségment*, with shwa.

4. A long syllable other than the one carrying the potential is often the syllable that carries the potential in a cognate or popularly associated word—the pitch accent here is an indirect source of length. Newman lists[33] many examples of this "underlying theme" relationship: *nàturalization* ∼ *náturalize; matèrialistic* ∼ *matérial*. In citation forms, and sometimes elsewhere, the syllable in question actually develops a secondary pitch accent.

If my assumption is correct, the speaker has the gradient covariable of duration at his command in any case of ambiguity: by reducing the syllable (and also centralizing the vowel somewhat), he throws the balance toward another syllable as the receiver of

[33] Pp. 184-185. I would add most if not all of his examples of "sonorous weak" (pp. 186-187) : *lĕgality* ∼ *légal, vĭbration* ∼ *vibrate, incŭlcation* ∼ *incúlcate*.

the accent. So in

who's
I don't know coming

either *know* or *who's* can be accented, depending on which is reduced in this fashion. Similarly

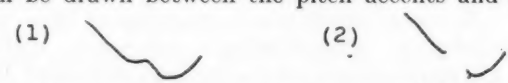
In
trigue

can be distinguished as to whether we have a speaker of one dialect saying *intrigue* with one kind of accent (Accent A; see next section) or a speaker of another dialect saying *intrigue* with another kind of accent (Accent C).

7. Pitch Accents A, B, and C

It would be possible to view pitch accent as a matter of obtrusion per se, relegating manners of approach, sustention, and take-off to another level of analysis, i.e., to intonation. This might seem the more advisable now that we know accent to be cued by the same kind of phonetic stimulus that we have always known operated in the sphere of intonation, namely, pitch change, and in view of the obvious difficulty of separating the different manners of achieving accent—apart from the fact of accent itself—from the domain of intonation.

That pitch can function in two equally complex ways at the same time, however, is well demonstrated by the tone-and-intonation languages. Chinese and Japanese use pitch phonemically, and despite this fact have intonations in many ways similar to those of English.[34] What happens in English with certain abrupt changes in a wider curve of pitch suggests that the same kind of line can be drawn between the pitch accents and intonation:



Two shapes like these are more alike, superficially, than they are different. Yet if we put an accentable syllable at the kink in the

[34] See Y. R. Chao, "A Preliminary Study of English Intonation (with American Variants) and its Chinese Equivalents," reprinted from *The Ts'ai Yüan P'ei Anniversary Volume* (Supplementary Vol. I of the *Bulletin of the Institute of History and Philology of the Academia Sinica*) (Peiping, 1932). See also Isamu Abe, "Intonational Patterns of English and Japanese," *Word*, XI (1955), 386-398. Weinreich, "Stress and Word Structure," treats stress in similar overlapping functions (phonemic, constructive, contrastive, expressive).

left-hand one and at the trough in the right-hand one, we get entirely different meanings:

(1) It wasn't Jo`ohn (2) It wasn't John

For a contrastive context in which *John* comes as a potentially new datum, (1) is appropriate: "I wish I knew who did a thing like that." — "It wasn't John (that much I'm sure of, though as to who else it might have been I'm not saying)." But (2) is uncomplicated denial; *John* is not a new datum: "I think it was John." —"It wasn't John—what are you talking about?" This contrast is pointed out by Maria Schubiger.[35] On the other hand, if we take a superficially disparate shape:

: It wasn't Jo`ohn

and put *John* at the peak, we again have implications of new-datum and contrast. The over-all similar curves carry something more dissimilar in meaning than the over-all dissimilar curves.

The phonetically similar fact in the two shapes with similar meanings is the way in which the accent is formed: it levels off from whatever kind of motion precedes it, and is followed by an abrupt drop. This differs from the other shape in that there the accent is formed by a drop TO the accentable syllable.

Now compare this all-or-none contrast (which is like the one on *intrigue* in the preceding section) with the gradient differences between

It was Jo`ohn It was Jo`ohn It was Jo\ohn It was Jo\ohn

which can be laid on a smooth scale of 'degree of finality'. The pitch differences between the first and last in the series are great, but the accents are the same. There is no point along this scale, as there was in the utterance of Test 1 and Test 10 in which the listeners had to make up their minds whether they were or were not hearing a given kind of accent on the word *easier*, where it is necessary to say "yes" or "no" rather than "more" or "less." The all-or-none is the domain of the pitch accents; the gradient is the domain of intonation.

[35] "Again: Fall-Rise Intonations in English," p. 2, reprinted from *English Studies*, XXXVII (1956). Her example is *I hadn't expected to see him there.* She credits "colleagues of University College, London," with having called the contrast to her attention.

Because each distinctive accent is thus capable of maintaining its identity despite broader intonational changes, I believe that it is practical to recognize more than the mere fact of prominence, of obtrusion per se, i.e., to describe and identify the different ways of achieving prominence. These are the pitch accents.

Test 13 undertakes to show identifications among similar manifestations of accent in dissimilar intonational settings, and discriminations between dissimilar accents in similar intonational settings. The experimenter arranged the phrase *I did* in the following four ways:

(1) I did (2) I di d (3) I di d (4) I di d

These were presented in 12 separately recorded groups of 3 as an ABX test, i.e., the listeners were asked to match X with whichever of the two preceding stimuli it resembled more.[36] Pattern (1) was present in each set of 3. Six listeners participated. A majority in every instance treated (1) as the orphan. This supports the theory that the four utterances have only two patterns of accent: in (1), the accent is formed by skipping down TO the accentable syllable. In (2), (3), and (4) it is formed by moving rapidly down FROM it.

There seem to be sufficient resemblances and sharp enough differences among accents to group them about three kinds of obtrusion, which I describe and label as follows:

Accent A: A relative leveling off of the accentable syllable followed by a relatively abrupt drop, either within the accentable syllable (which is prolonged for the purpose) or in the immediately following syllable. In very rapid speech the drop may be postponed to the second following syllable, but rarely beyond this. There are affective (intonational) differences between a drop within the accentable syllable and a drop later.

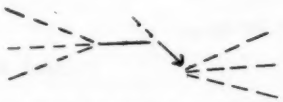
The commonest type of approach ("head" in Palmer's terminology, "precontour" in Pike's) to the A accent in American English is from a lower pitch, with a skip up to the accentable syllable. In British Received Pronunciation, if descriptions are accurate, an approach from a higher pitch would seem to be at

[36] For an example of ABX procedure, see Liberman, Harris, Hoffman, and Griffith, "The Discrimination of Speech Sounds Within and Across Phoneme Boundaries," *Journal of Experimental Psychology*, LIV (1957), 358-368.

least as common (the affective nuances need to be correlated as much with dominant social attitudes as with "dialect"). One rather sharp deviation, which for semantic reasons I would class as a sub-type of A, puts the accentable syllable at a lower pitch than the one immediately following, but requires that only that one weak syllable remain high—the syllable after it must come down rapidly. An example from a radio dramatization,[37] with accent on *out*:

I am seldom out of my castle

The least common denominator in all A's is the abrupt fall rarely more than two syllables after the accentable syllable. In the following diagram the arrow represents a skip or skip-like motion, and solid lines denote essential movements while dotted lines indicate optional ones:



Accent C: This is a kind of anti-accent A, both in form and in meaning. The accentable syllable is approached from above, and skipped down to. What follows may level off or rise, but a further fall seems to be avoided:

Accent B: The characteristic of this accent is upmotion. It is neither skipped down to nor skipped down from. It may be approached from below and skipped up to, with the following motion continuing level, or rising (the usual thing), or falling slightly (an abrupt drop would create an A). Or it may be approached from a relative level and skipped up from, after which the movement usually continues upward slightly or levels off. This makes two diagrams necessary:

[37] Charles Dickens, *The Poor Relation*, broadcast from Station WABC, New York, 7:55 P. M., December 24, 1956.

Pike's ²4-3-4 contour (pp. 56-57) is an example of this type of A accent compressed on a single syllable, *yes* and *no*, prolonged for the purpose.

Examples of the accents:

B
Do you real^ly hate your brother?

B
Do you really hate your brother?

B
Do you really hate your brother?

B
Do you really hate your brother?

A
Do you real^ly hate your brother?

} Minimal contrast between A and B

A
Do you real^ly hate your brother?

A A
Do you real^ly hate your bro^ther?

A
C bro
Do you really hate your ^ther? (As if impatiently repeated)

C
Do you really hate your C
brother?

B B
Do you really hate your bro^ther?

B A
Do you really hate your bro^ther?

B A
Do you really hate your bro^ther? (An echo question)

8. The Accents as Morphemes

The procedure that I have followed in grouping the accents about certain norms has been first to look for similarities and differences in meaning, and then to try to match them with similarities and differences in form. This reverses the approved order of business, but had to be adopted because pitch contours are if anything more fluid than meanings.

To take meaning as the starting point can be justified only if the accents are meaningful units. It assumes that they are morphemes, not phonemes. This assumption holds even in the most restricted sense of accent as mere fact-of-prominence. It opposes the current American theory, which is that stresses are meaningless sub-units that build themselves into morphemes ("superfixes") which give, in effect, a phonemic shape to syntax. I regard this view as untenable.[38] The older theory, that accent signals emphasis, has been re-stated by Weinreich for Yiddish: phonemic stress within the morpheme (i.e., word stress) is "the place at which relative loudness [sc. pitch change] occurs if the morpheme is emphasized."[39] This is to say that stress is phonemic only in the sense that a given syllable and not some other syllable within the morpheme carries the potential for pitch accent. When the accent occurs, it signals emphasis, i.e., is meaningful.

Accent A is assertive. It is used with items that are separately important, contrastive, and/or new to the discourse. It usually singles out the morph on which it falls, but this function of separation ("contour separation" in Pike's terminology) may be absent. I have heard a single syllable given as many as three successive A accents:[40]

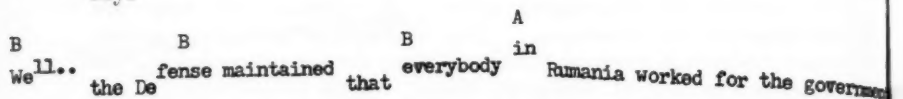
Also, instead of singling out one item, the A accent may put em-

[38] See my "Stress and Information," *American Speech*, XXXIII (1958), 5-20, and "Intonation and Grammar," *Language Learning*, VIII (1957-58), 31-38.

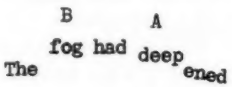
[39] Weinreich, "Stress and Word Structure," p. 2. The relevant part of this statement is that accent is not phonemic, whereas potential for pitch accent is. Whether Weinreich is right in not admitting any other kind of word stress than the potential is another matter. It has been disputed by Stockwell, *Language*, XXXII (1956), 374-383.

[40] This freedom to "spread" syllables in order to accommodate more accents reflects a certain degree of freedom in the location of the potential for pitch accent in English. Not only is the normal locus of the potential free, i. e. unpredictable in terms of the

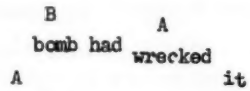
phasis on the utterance as a whole. For this purpose, an item that is ordinarily incapable of receiving it is selected—an auxiliary verb, a preposition, or something of the sort. In the following example there is no contrast between 'in' and 'out of', nor is there any special attention of any other kind bestowed on the word *in*; it merely carries a sentence accent which, if it were to fall anywhere else, might be mistaken as contrastive: the speaker refers to someone who had been denied re-entry to the United States because of having worked for the Communist government of Rumania, and says



The 'separateness' and 'newness' of A may be illustrated by what happens in a narrative. If one is telling a story in which a fog has been introduced, at a later point one may say



But a B accent for something new and unexpected is unnatural. If we encountered



at all, it would probably be in a time of violence when bombs are

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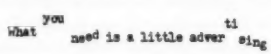
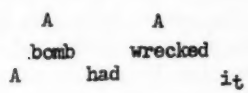
total number of syllables and mechanical placement (in Arnold's terms, p. 440, "TONIC STRONG stress is undoubtedly free and not tied to any given position within the word"), but also other syllables may be raised to the status of potential-carrier when it is desired to lay on more than one accent. This is especially true when one word occupies the dimensions of a whole utterance. Note the successions of AAA and CCA in the following:

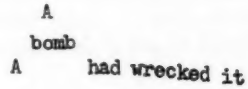
It also operates to shift the potential toward the end of a word when the word occupies the climactic position in the sentence, as when Thomas Mitchell said, in a TV program,



commonplace. For something as inherently unlikely as a bombing,

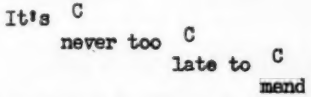


with two A accents, or

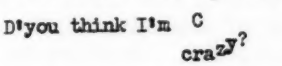


with one A accent, on *bomb* itself, is more probable.

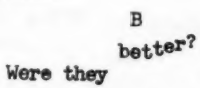
Accent C is anti-assertive. It may be merely lackadaisical, as in a descending series like



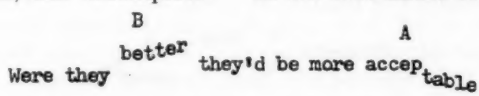
or it may be clearly and even strenuously restrained, as in



Accent B means something like 'connectedness' and 'incompleteness.' In the question



it is terminal, and 'incomplete'. In the first clause of



it is non-terminal and 'connected' to the following Accent A.

Test 14 relates this connectedness to compound words. A live-voice recording of *attorney-at-law* was made. The natural pitch pattern was removed, and the following artificial ones substituted :

(1) (90) *at*(110)*tor*(90)*ney-at-*(110-90)*law*
(2) (90) *at*(110)*torney-at-*(110-90)*law*
(3) (90) *at*(110)*torney-*(90)*at-*(110-90)*law*

The stimuli were mixed with others involving *mother-in-law*, and

played to a group of seven listeners, who were asked to distinguish between those that seemed properly stressed and those that did not. Six approved (2) and (3), which have the B accent on *-lor-* that seems to characterize this kind of compound. Only three approved (1) with its A accent on *-lor-*.[41]

Much more testing needs to be done to make these definitions anything more than rough approximations. They are probably close enough to the truth, however, to credit separate semantic functions to separate kinds of pitch prominence.

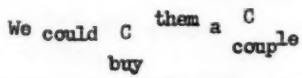
9. Accent and Intonation

The pitch accents do not eliminate the problem of intonation. They merely narrow it by cutting out certain all-or-none contrasts. The residue is mostly gradient. Differences of meaning attach to:

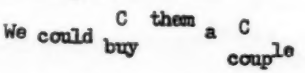
1. Steep falls as opposed to gradual falls.

2. High approaches to a falling A accent as opposed to lower approaches.

3. In a succession of A accents that are skipped up to, the relative height of the peaks. Similarly for the relative depth of the troughs of successive C accents; note the difference between the "bright idea" of



with an upward tangent to the troughs, and the more subdued implication where the tangent is down:



4. A falling approach to a C accent as opposed to a high-pitched approach with steeper downskip:

Others could be added.

[41] Further contrasts of A and B have been described in "Certain Functions of Accents A and B." In addition, other tests have shown both B and C to be inappropriate to a contrastive context.

Furthermore, the problem of terminal junctures needs to be re-studied in the light of pitch accent. Something like the endings that Smith and Trager describe is probably correct, but with refinements. For example, it seems to make little difference after a B accent whether there is a slight fall, a level, or a rise; the differences here are gradient. But after an A accent there seems to be an all-or-none difference between a level and a rise, but a gradient difference between a level and a fall. In answer to *George asked for his shovel back* one might get

A

gave

I thought he it to you

with *to you* at a fairly high tonal level, as if the speaker were wondering and half-questioning; but if there is a rise in pitch on *you*, the utterance either takes on an admonitory tone or, though these words scarcely lend themselves, becomes a perplexed iterative question. On the other hand, if *to you* remains level, it may stand at any height, with gradient differences of suspension and conclusiveness; or it may fall for still greater conclusiveness, but without the sharp difference created by the rise.

10. Conclusions

1. Tests with both natural and artificial speech have shown that the primary cue of what is usually termed STRESS in the utterance is pitch prominence.

2. Intensity is found to be negligible both as a determinative and as a qualitative factor in stress.

3. To avoid unwanted associations, it is better to speak of PITCH ACCENT and to leave the term STRESS to the domain of word stress. In the latter domain, one possible kind of phonemic stress is POTENTIAL FOR PITCH ACCENT.

4. While the upward obtrusion is basic, pitch prominence need not be merely upward, as commonly supposed, but may take other directions. The differences of form respond to differences of meaning, giving the accents a morphemic status. Differentiating them often calls for a repertorial cue (the user's knowledge of the morphs of his language, and what syllables have the potential for pitch accent) or a gradient phonetic cue (length of syllable and grade of vowel).

University of Southern California.

THE CHILD'S LEARNING OF ENGLISH MORPHOLOGY

JEAN BERKO

In this study[1] we set out to discover what is learned by children exposed to English morphology. To test for knowledge of morphological rules, we use nonsense materials. We know that if the subject can supply the correct plural ending, for instance, to a noun we have made up, he has internalized a working system of the plural allomorphs in English, and is able to generalize to new cases and select the right form. If a child knows that the plural of *witch* is *witches*, he may simply have memorized the plural form. If, however, he tells us that the plural of **gutch* is **gutches*, we have evidence that he actually knows, albeit unconsciously, one of those rules which the descriptive linguist, too, would set forth in his grammar. And if children do have knowledge of morphological rules, how does this knowledge evolve? Is there a progression from simple, regular rules to the more irregular and qualified rules that are adequate fully to describe English? In very general terms, we undertake to discover the psychological status of a certain kind of linguistic description. It is evident that the acquisition of language is more than the storing up of rehearsed utterances, since we are all able to say what we have not practiced and what we have never before heard. In bringing descriptive linguistics to the study of language acquisition, we hope to gain knowledge of the systems and patterns used by the speaker.

In order to test for children's knowledge of this sort, it was necessary to begin with an examination of their actual vocabulary. Accordingly, the 1000 most frequent words in the first-grader's vocabulary were selected from Rinsland's listing.[2] This listing

[1] This investigation was supported in part by a fellowship from the Social Science Research Council. During the academic year 1957-58 the writer completed the research while holding an AAUW National Fellowship. A dissertation on this subject was presented by the writer to Radcliffe College in April, 1958. I am indebted to Professor Roger W. Brown for his inspiration and his help in the conduct of this study.

[2] H. D. Rinsland, *A Basic Vocabulary of Elementary School Children*, New York, MacMillan, 1945.

contains the most common words in the elementary school child's vocabulary, as taken from actual conversations, compositions, letters, and similar documents. This list was then examined to see what features of English morphology seem to be most commonly represented in the vocabulary of the first-grade child. From this we could decide what kind of extensions we might expect the child to be able to make. All of the English inflexional morphemes were present.

The areas that seemed to be most promising from this examination were the plural and the two possessives of the noun, the third person singular of the verb, the progressive and the past tense, and the comparative and superlative of the adjective. The pronouns were avoided both because of the difficulty involved in making up a nonsense pronoun, and because the pronouns are so few in number and so irregular that we would hardly expect even adults to have any generalized rules for the handling of new pronouns. Moreover, we do not encounter new pronouns, whereas new verbs, adjectives, and nouns constantly appear in our vocabularies, so that the essential problem is not the same. The past participle of regular or weak verbs in English is identical with the past tense, and since the regular forms were our primary interest, no attempt was made to test for the past participle. A number of forms that might suggest irregular plurals and past tenses were included among the nouns and verbs.

The productive allomorphs of the plural, the possessive, and the third person singular of the verb are phonologically conditioned and identical with one another. These forms are /-s ~ -z ~ -əz/, with the following distribution:

/-əz/ after stems that end in/s z š ž č ǰ/, e.g. *glasses*, *watches;*

/-s/ after stems that end in /p t k f θ/, e.g. *hops*, *hits;*

/-z/ after all other stems, viz. those ending in /b d g v ð m n ŋ r l/, vowels, and semivowels, e.g. *bids*, *goes*.

The productive allomorphs of the past are /-t ~ -d ~ -əd/, and they are also phonologically conditioned, with the following distribution:

/-əd/ after stems that end in /t d/, e.g. *melted;*

/-t/ after stems that end in /p k č f θ š/, e.g. *stopped;*

/-d/ after stems ending in voiced sounds except /-d/, e.g. *climbed*, *played*.

The progressive *-ing* and the adjective *-er* and *-est* do not have variants. It might also be noted that the possessive has an

additional allomorph /-ø/; this occurs after an inflexional /-s/ or /-z/, so that if the form *boy* is made plural, *boys*, the possessive of that plural form is made by adding nothing, and indicated in writing only by the addition of an apostrophe: *boys'*.

The children's vocabulary at the first-grade level also contains a number of words that are made of a free morpheme and a derivational suffix, e.g. *leacher*, or of two free morphemes, e.g. *birthday*. The difficulties encountered in this area are many. First, it might be noted that there are not many contrasts, i.e., not many cases of the same derivational suffix being added to different bases to produce forms of like function. Although *beautiful* and *thankful* both appear on the list, it does not seem that these examples are numerous enough for us to expect a young child to be able to append *-ful* to a new noun in order to produce an adjective. Word derivation and compounding are furthermore often accompanied by changes in stress and pronunciation, so that the picture is additionally complicated. There seemed to be enough examples of the stress pattern ´ `, as in *blackboard* as against *black board*, and of the diminutive-affectionate *-y*, the adjectival *-y*, and the agentive *-er* to warrant testing for these forms.

So far as the general picture is concerned, all speakers of the language are constrained to use the inflexional endings and apply them appropriately to new forms when they are encountered. We are not so often called upon to derive or compound new words, although by the time we are adults we can all to some extent do this. From the children's actual vocabulary we were able to make an estimate of the kind of morphological rules they might be expected to possess, and from these items a test could be constructed. It was noted, moreover, that in the child's vocabulary there are a number of compound words, like *blackboard* and *birthday*. It is entirely possible to use a compound word correctly and never notice that it is made of two separate and meaningful elements. It is also possible to use it correctly and at the same time have a completely private meaning for one or both of its constituent elements. In order to see what kind of ideas children have about the compound words in their vocabularies, it was decided to ask them directly about a selected number of these words.

Within the framework of the child's vocabulary, a test was devised to explore the child's ability to apply morphological rules to new words. He was called upon to inflect, to derive, to compound, and, lastly, to analyse compound words.

Materials and procedure

In order to test for the child's use of morphological rules of different types and under varying phonological conditions, a number of nonsense words were made up, following the rules for possible sound combinations in English. Pictures to represent the nonsense words were then drawn on cards. There were 27 picture cards, and the pictures, which were brightly colored, depicted objects, cartoon-like animals, and men performing various actions. For reasons that will be discussed later, several actual words were also included. A text, omitting the desired form, was typed on each card. An example of the card to test for the regular plural allomorph in /-z/ can be seen in figure 1.

The subjects included 12 adults (seven women and five men), all of whom were college graduates. Many of these adults had also had some graduate training. All were native speakers of English.

The child subjects were obtained at the Harvard Preschool in Cambridge and the Michael Driscoll School, in Brookline, Massachusetts. At the Preschool, each child was brought to the experimenter, introduced, and told that now he was going to look at some pictures. The experimenter would point to the picture and read the text. The child would supply the missing word, and the item he employed was noted phonemically. After all of the pictures had been shown, the child was asked why he thought the things denoted by the compound words were so named. The general form of these questions was "Why do you think a blackboard is called a blackboard?" If the child responded with "Because it's a blackboard", he was asked, "But why do you think it's called that?" The children at the preschool ranged between four and five years in age. Twelve girls and seven boys were asked all items of the completed test, and two groups, one of three boys and three girls and one of five boys and three girls, were each asked half of the inflexional items in preliminary testing.

At the Driscoll School, the experimenter was introduced to the class and it was explained that each child was going to have a turn at looking at some pictures. The procedure from this point on was the same as for the Preschool. All children in the first grade were interviewed. There were 26 boys and 35 girls in this group. Ages ranged from five and one half to seven years.

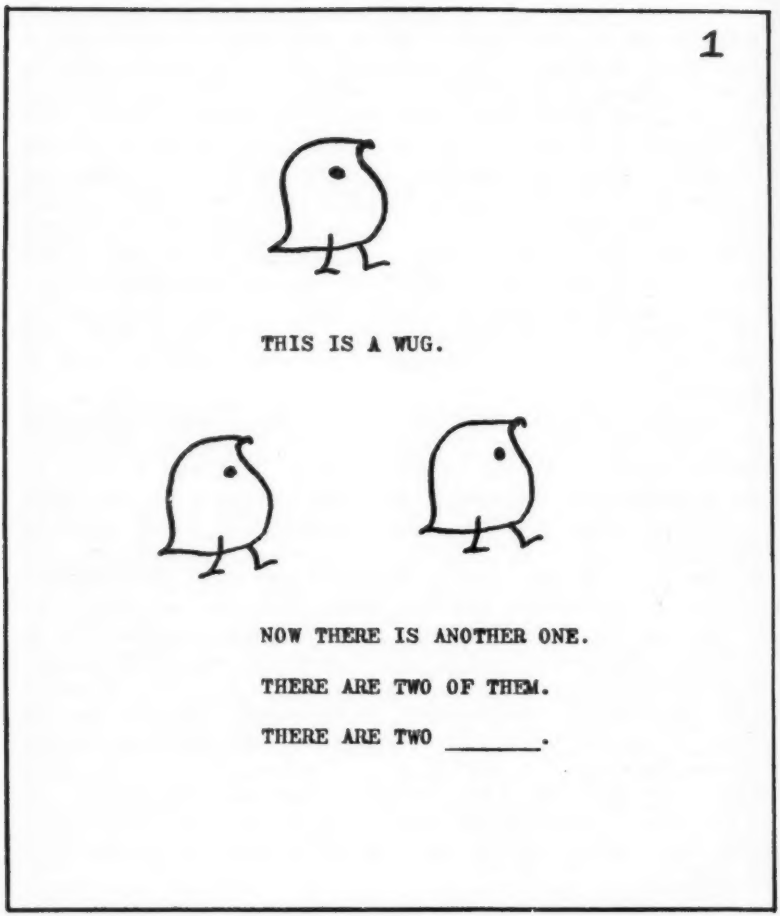


Figure 1. The plural allomorph in /-z/.

The following is the order in which the cards were presented. Included is a statement of what was being tested, a description of the card, and the text that was read. Pronunciation is indicated by regular English orthography; a phonemic transcription is included for first occurrences of nonsense words.

1. Plural. One bird-like animal, then two. "This is a wug /wʌg/. Now there is another one. There are two of them. There are two ——·"

2. Plural. One bird, then two. "This is a gutch /gʌč/. Now there is another one. There are two of them. There are two ——."

3. Past tense. Man with a steaming pitcher on his head. "This is a man who knows how to spow /spow/. He is spowing. He did the same thing yesterday. What did he do yesterday? Yesterday he ——."

4. Plural. One animal, then two. "This is a kazh /kæž/. Now there is another one. There are two of them. There are two ——·"

5. Past tense. Man swinging an object. "This is a man who knows how to rick /rik/. He is ricking. He did the same thing yesterday. What did he do yesterday? Yesterday he ——."

6. Diminutive and compounded or derived word. One animal, then a miniscule animal. "This is a wug. This is a very tiny wug. What would you call a very tiny wug? This wug lives in a house. What would you call a house that a wug lives in?"

7. Plural. One animal, then two. "This is a tor /tɔr/. Now there is another one. There are two of them. There are two ——."

8. Derived adjective. Dog covered with irregular green spots. "This is a dog with quirks /kwərks/ on him. He is all covered with quirks. What kind of dog is he? He is a —— dog."

9. Plural. One flower, then two. "This is a lun /lʌn/. Now there is another one. There are two of them. There are two ——·"

10. Plural. One animal, then two. "This is a niz /niz/. Now there is another one. There are two of them. There are two ——·"

11. Past tense. Man doing calisthenics. "This is a man who

knows how to mot /mat/. He is motting. He did the same thing yesterday. What did he do yesterday? Yesterday he ——·"

12. Plural. One bird, then two. "This is a cra /kra/. Now there is another one. There are two of them. There are two ——·"

13. Plural. One animal, then two. "This is a tass /tæs/. Now there is another one. There are two of them. There are two ——·"

14. Past tense. Man dangling an object on a string. "This is a man who knows how to bod /bad/. He is bodding. He did the same thing yesterday. What did he do yesterday? Yesterday he ——·"

15. Third person singular. Man shaking an object. "This is a man who knows how to naz /næz/. He is nazzing. He does it every day. Every day he ——·"

16. Plural. One insect, then two. "This is a heaf /hiyf/. Now there is another one. There are two of them. There are two ——·"

17. Plural. One glass, then two. "This is a glass. Now there is another one. There are two of them. There are two ——·"

18. Past tense. Man exercising. "This is a man who knows how to gling /gliŋ/. He is glinging. He did the same thing yesterday. What did he do yesterday? Yesterday he ——·"

19. Third person singular. Man holding an object. "This is a man who knows how to loodge /luwdž/. He is loodging. He does it every day. Every day he ——·"

20. Past tense. Man standing on the ceiling. "This is a man who knows how to bing /biŋ/. He is binging. He did the same thing yesterday. What did he do yesterday? Yesterday he ——·"

21. Singular and plural possessive. One animal wearing a hat, then two wearing hats. "This is a niz who owns a hat. Whose hat is it? It is the —— hat. Now there are two nizzes. They both own hats. Whose hats are they? They are the —— hats."

22. Past tense. A bell. "This is a bell that can ring. It is ringing. It did the same thing yesterday. What did it do yesterday? Yesterday it ——·"

23. Singular and plural possessive. One animal wearing a hat, then two. "This is a wug who owns a hat. Whose hat is it? It is the —— hat. Now there are two wugs. They both own hats. Whose hats are they? They are the —— hats."

24. Comparative and superlative of the adjective. A dog with a few spots, one with several, and one with a great number. "This dog has quirks on him. This dog has more quirks on him. And this dog has even more quirks on him. This dog is quirky. This dog is ——. And this dog is the ——."

25. Progressive and derived agentive or compound. Man balancing a ball on his nose. "This is a man who knows how to zib /zib/. What is he doing? He is ——. What would you call a man whose job is to zib?"

26. Past tense. An ice cube, then a puddle of water. "This is an ice cube. Ice melts. It is melting. Now it is all gone. What happened to it? It ——."

27. Singular and plural possessive. One animal wearing a hat, then two. "This is a bik /bik/ who owns a hat. Whose hat is it? It is the —— hat. Now there are two biks. They both own hats. Whose hats are they? They are the —— hats."

28. Compound words. The child was asked why he thought the following were so named. (No pictures were used for these items.)

a. afternoon
b. airplane
c. birthday
d. breakfast
e. blackboard
f. fireplace
g. football
h. handkerchief
i. holiday
j. merry-go-round
k. newspaper
l. sunshine
m. Thanksgiving
n. Friday

It took between ten and fifteen minutes to ask a child all of these questions. Even the youngest children have had experience with picture books, if not actual training in naming things through pictures, and no child failed to understand the nature of the task before him. It was, moreover, evident that a great number of these children thought they were being taught new English words. It was not uncommon for a child to repeat the nonsense word immediately upon hearing it and before being asked any questions. Often, for example, when the experimenter said "This is a *gutch*", the child repeated, "*Gutch*". Answers

were willingly, and often insistently, given. These responses will be discussed in the following section.

Results

Adult answers to the inflexional items were considered correct answers, and it was therefore possible to rate the children's answers. In general, adult opinion was unanimous—everyone said the plural of **wug* was **wugs*, the plural of **gutch* was **gutches;* where the adults differed among themselves, except in the possessives, it was along the line of a common but irregular formation, e.g. **heaf* became **heaves* in the plural for many speakers, and in these cases both responses were considered correct. If a child said that the plural of **heaf* was **heafs* or **heaves* /-vz/, he was considered correct. If he said **heaf* (no ending), or **heafès* /-fəz/, he was considered incorrect, and a record was kept of each type of response.

Sex Differences

The first question to be answered was whether there is a sex difference in the ability to handle English morphology at this age level. Since it seemed entirely possible that boys entering the first grade might be on the whole somewhat older than girls entering the first grade, it was necessary to equate the two groups for age.

Table 1. Distribution of children at each age level for comparison of the sexes

Age	*Boys*	*Girls*	*Total*
4	2	2	4
4:6	1	1	2
5	2	2	4
5:6	2	2	4
6	10	10	20
6:6	6	6	12
7	5	5	10
Total:	28	28	56

The children were divided into seven age groups. Since at each of these levels there were more girls than boys, a random selection of the girls was made so that they would match the boys in number. The distribution of these ages and the number in

each group can be seen in Table 1. This distribution was utilized only in comparing the performance of the boys with that of the girls; in all other instances, the responses of the entire sample were considered.

The groups of 28 boys and 28 girls thus selected were compared with one another on all inflexional items. The chi square criterion with Yates' correction for small frequencies was applied to each item, and on none was there a significant difference between the boys' and girls' performance; boys did as well as girls, or somewhat better, on over half the items, so that there was no evidence of the usual superiority of girls in language matters. From this it would appear that boys and girls in this age range are equal in their ability to handle the English morphology represented by these items.

Age Differences

Having ascertained that there was no difference between boys' and girls' answers, we combined the sexes and went on to compare the younger with the older children. The oldest children at the Preschool were five years old, and the youngest at the Driscoll School were five and one half years, so that the dividing line was made between the schools. Chi square corrected for small frequencies was again applied to all inflexional items. First graders did significantly better than preschoolers on slightly less than half of these. The differences can be seen in Table 2.

Table 2. Age differences on inflexional items

Item	Percentage of correct pre-school answers	Percentage of correct first-grade answers	Significance level of difference
Plural			
glasses	75	99	.01
wugs	76	97	.02
luns	68	92	.05
tors	73	90	—
heafs	79	80	—
cras	58	86	.05
tasses	28	39	—
gutches	28	38	—
kazhes	25	36	—
nizzes	14	33	—

Item	Percentage of correct pre-school answers	Percentage of correct first grade answers	Significance level of difference
Progressive			
zibbing	72	97	.01
Past Tense			
binged	60	85	.05
glinged	63	80	—
ricked	73	73	—
melted	72	74	—
spowed	36	59	—
motted	32	33	—
bodded	14	31	.05
rang	0	25	.01
Third Singular			
loodges	57	56	—
nazzes	47	49	—
Possessive			
wug's	68	81	—
bik's	68	95	.02
niz's	58	46	—
wugs'	74	97	.02
biks'	74	99	.01
nizzes'	53	82	.05

FORMATION OF THE PLURAL.

The nature of the children's answers can best be seen through a separate examination of the noun plurals, the verbs, and the possessives. The percentage of all children supplying correct plural endings can be seen in Table 3. The general picture indicates that children at this age have in their vocabularies words containing the three plural allomorphs /-s ∼ -z ∼ -əz/, and can use these words. The real form *glasses* was included here because we knew from a pretest that children at this age generally did not make correct application of /-əz/ to new forms, and we wanted to know if they used this form with a common English word. Evidently they have at least one actual English model for this contingent plural. In uncomplicated cases children at this age can also extend the use of these forms to new words requiring

/-s/ or /-z/, as indicated by the high percentage of right answers for **wug* and **bik*, a form used in the pretest and answered correctly by a correspondingly high number of children. For the items **wugs* and *glasses* there is, moreover, a significant difference between the younger and older groups. For *glasses* they progress from 75 % right to 99 % right in the older group, a change that is significant at the 1 % level. The few wrong answers in these cases were either a complete failure to respond, or a repetition of the word in its singular form.

TABLE 3. PERCENTAGES OF CHILDREN SUPPLYING CORRECT PLURAL FORMS

Item	*Allomorph*	*Per Cent Correct*
glasses	/-əz/	91
wugs	/-z/	91
luns	/-z/	86
tors	/-z/	85
heafs, -ves	/-s/ /-z/	82
cras	/-z/	79
tasses	/-əz/	36
gutches	/-əz/	36
kazhes	/-əz/	31
nizzes	/-əz/	28

From this it is evident that however poorly children may do on extensions of the rule for forming the plural of *glass*, they do have this item in their vocabulary and can produce it appropriately. During the period from preschool to the first grade, those who do not have this item acquire it. They can also extend the rule for the addition of the /-s/ or /-z/ allomorph where the more general rules of English phonology dictate which of these forms must be used. During this period they perfect this knowledge.

The ability to add /-z/ to **wug* and /-s/ to **bik* does not alone prove that the child possesses the rule that tells which allomorph of the plural must be used: English phonology decrees that there cannot be a consonant cluster */-kz/ or */-gs/. The final consonant determines whether the sibilant must be voiced or unvoiced. The instances in English where there is a choice are after /l/ /n/ and /r/, and after a vowel or semivowel. Thus we have minimal pairs like: *ells : else; purrs : purse; hens : hence; pews : puce.* In forming the plural of **wug* or **bik*, the child has only to know

that a dental sibilant must be added; which one it is is determined by the invariant rules of combination that govern English consonant clusters. If, however, he is faced with a new word ending in a vowel, semivowel, /-l/, /-n/, or /-r/, he himself must make the choice, because so far as English phonology is concerned he could add either a /-z/ or an /-s/ and still have a possible English word. We would expect him, therefore, to have more difficulty forming the plural of a new word ending in these sounds than in cases where phonology determines the form of the sibilant. These problems are represented by the forms **cra*, **tor*, and **lun*. As table 3 indicates, the percentages correct on these items were respectively 79, 85, and 86. The difference between performance on **wug* and **cra* is significant at the 5 % level.

During the period from preschool to the first grade, they improved markedly in their handling of **cra* and **lun*. The differences between the younger and older groups were significant at the 5 % level. The case of adding /-s/ to these forms did not, however, arise. The child here, as in so many other stages of language learning, answered complexity with silence: the wrong answers were invariably the unaltered form of the singular.

The only other case to be answered correctly by the majority of the children was **heaf*. Since adults responded with both **heafs* and **heaves* /-vz/, both of these answers were considered correct. It must be noted that although 42 % of the adults gave **heaves* as the plural of this item, employing what would amount to a morphophonemic change along the lines of: *knife*: *knives; hoof*: *hooves*, only three children out of a total of 89 answering this item said **heaves;* 9, or 10 % added nothing, and an additional four formed the plural with the wrong allomorph, i.e. they said /hiyfəz/, treating the /-f/ as if it belonged to the sibilant-affricate series. /f/ is, of course, phonetically very similar to /s/, and one of the questions suggested by this problem was whether children would generalize in the direction of phonetic similarity across functional boundaries—/f/ is distinguished phonetically from /s/ only in that it is grave and /s/ is acute. It is, so to speak, no more different from /s/ than /z/ is, and it is as similar to /s/ as /ž/ is to /z/. It does not, however, so far as English phonology is concerned, function like /s š z ž č ǰ/, none of which can be immediately followed by another sibilant within the same consonant cluster. The high percentage of correct items indicates that /f/ had already been categorized as belonging to the consonant class that can be followed by /-s/, and the phonetic similarity

between /f/ and the sibilants did not lead the children to generalize the rule for the addition of the /-əz/ allomorph in that direction. Nor could any irregular formation be said to be productive for children in this case, although for adults it apparently is.

The proportion of children's right answers suddenly drops when we come to the form **tass*. As table 3 shows, 91 % of these children when given the form *glass* could produce the form *glasses*. When given the form **tass*, a new word patterned after *glass*, only 36 % could supply the form **tasses*. The picture becomes progressively worse with the other words ending in sibilants or affricates, and by the time we reach the form **niz*, only 28 % answered correctly. **Niz* of these four, is the only one that ends in a sound that is also the commonest plural allomorph, /-z/, and the children did the worst on this item. What is of additional interest, is that on these four items there was no significant improvement from the preschool to the first grade. The difference between performance on **cra*, the worst of the other items, and **tass*, the best of these, was significant at the .1 % level. Again, the wrong answers consisted in doing nothing to the word as given. It must be noted, however, that in these items, the children delivered the wrong form with a great deal of conviction: 62 % of them said "one **tass*, two **tass*" as if there were no question that the plural of **tass* should and must be **tass*. From this it is evident that the morphological rules these children have for the plural are not the same as those possessed by adults: the children can add /-s/ or /-z/ to new words with a great deal of success. They do not as yet have the ability to extend the /-əz/ allomorph to new words, even though it has been demonstrated that they have words of this type in their vocabulary.

The form "kazh" /kæž/ was added here once again to see in what direction the children would generalize. /ž/, although it is in the sibilant-affricate group, is very rare as a final consonant in English: it occurs only in some speakers' pronunciation of *garage*, *barrage*, and a few other words. As table 3 indicates, the children treated this word like the others of this group. It might also be noted here that for the forms **gutch* and **kazh*, some few children formed the plural in /-s/, i.e., /gʌčs/ and /kæžs/. 10 % did this for **gutch*, and 5 % for **kazh*, errors that indicate that the phonological rules may not yet be perfectly learned. What is clearest from these answers dealing with the plural is that children can and

do extend the /-s/ and /-z/ forms to new words, and that they cannot apply the more complicated /-əz/ allomorph of the plural to new words.

Verb Inflexions

The children's performance on the verb forms can be seen in Table 4. It will be observed that the best performance on these items was on the progressive, where they were shown a picture of a man who knew how to **zib* and were required to say that he was **zibbing*. The difference between **zibbing* and the best of the past tense items, **binged*, was significant at the 5 % level. The improvement from the younger to the older group was significant at the 1 % level; fully 97 % of the first graders answered this question correctly. Here, there was no question of choice, there is only one allomorph of the progressive morpheme, and the child either knows this *-ing* form or does not. These results suggest that he does.

The results with the past tense forms indicate that these children can handle the /-t/ and /-d/ allomorphs of the past. On **binged* and **glinged* the percentages answering correctly were 78 and 77, and the older group did significantly better than the younger group on **binged*.

Table 4. Percentages of children supplying correct verb forms

Item	*Allomorph*	*Percentage Correct*
Progressive		
zibbing	/-iŋ/	90
Past Tense		
binged, bang	/-d ∾ æ ← (i)/	78
glinged, glang	/-d ∾ æ ← (i)/	77
ricked	/-t/	73
melted	/-əd/	73
spowed	/-d/	52
motted	/-əd/	33
bodded	/-əd/	31
rang	/æ←(i)/	17
Third Singular		
loodges	/-əz/	56
nazzes	/-əz/	48

Actually, the forms **gling* and **bing* were included to test for possible irregular formations. A check of English verbs revealed that virtually all in *-ing* form their past tense irregularly: *sing: sang; ring: rang; cling: clung*, and many others. The only *-ing* verbs that form a past tense in *-ed* are a few poetic forms like *enringed*, *unkinged*, and *winged*, and onomotopoeias like *pinged* and *zinged*. Adults clearly felt the pull of the irregular pattern, and 50 % of them said **bang* or **bung* for the past tense of **bing*, while 75 % made **gling* into **glang* or **glung* in the past. Only one child of the 86 interviewed on these items said **bang*. One also said **glang*, and two said **glanged*—changing the vowel and also adding the regular /-d/ for the past.

The great majority on these forms, as well as on **ricked* which requires /-t/, formed the past tense regularly. There was a certain amount of room for variation with the past tense, since there is more than one way of expressing what happened in the past. A number of children, for example said "Yesterday he was **ricking*". If on these occasions the experimenter tried to force the issue by saying "He only did it once yesterday, so yesterday once he—?" The child usually responded with "once he was **ricking*". Taking into account this possible variation, the percentages right on **rick*, **gling* and **bing* represent a substantial grasp of the problem of adding a phonologically determined /-t/ or /-d/.

With **spow* the child had to choose one or the other of the allomorphs, and the drop to 52 % correct represents this additional complexity. Several children here retained the inflexional /-z/ and said /spowzd/, others repeated the progressive or refused to answer. No child supplied a /-t/.

On **molled*, the percentage correct drops to 33, although the subjects were 73 % right on the real word *melled*, which is a similar form. On **bodded* they were 31 % right, and on *rang* only 17 % right. The older group was significantly better than the younger on *rang* and **bodded*. What this means is that the younger group could not do them at all—not one preschool child knew *rang*—and the older group could barely do them. What emerges here is that children at this age level are not able to extend the rule for forming the past tense of *melled* to new forms. They can handle the regular /-d/ and /-t/ allomorphs of the past in new instances, but not /-əd/. Nor do they have control of the irregular past form *rang*, and consequently do not form new pasts according to this pattern, although adults do. They have the /-əd/ form in actual

words like *melted*, but do not generalize from it. With *ring*, they do not have the actual past *rang*, and, therefore no model for generalization. In the children's responses, the difference between **spowd*, the worst of the items requiring /-t/ or /-d/, and **molted*, the best requiring /-əd/ is significant at the 2 % level. For **mot* and **bod*, the wrong answers, which were in the majority, were overwhelmingly a repetition of the present stem: "Today he* *bods*; yesterday he **bod*." To the forms ending in /-t/ or /-d/ the children added nothing to form the past.

The third person singular forms require the same allomorphs as the noun plurals, /-s∿-z∿-əz/, and only two examples were included in the experiment. These were **loodge* and **naz*, and required the /-əz/ ending. 56 % of the children supplied the correct form **loodges*, and 48 % supplied **nazzes*. The wrong answers were again a failure to add anything to the stem, and there was no improvement whatsoever from the younger to the older group on these two items.

Formation of the Possessive

The only other inflexional items statistically treated were the regular forms of the possessive. The percentages of children supplying right answers can be seen in Table 5. In the singular, the problem was the same as for the noun plurals, and the children's difficulty with the /-əz/ form of the allomorph is mirrored in the low percentage who were able to supply **niz's* /-əz/ when told "This is a *niz* who owns a hat. Whose hat is it? It is the ———?" For **bik's* there was a significant improvement at the 2 % level between the younger and older groups. For **niz's* the younger group did no worse than the older group.

In the plural possessives the problem is somewhat different: since these words are already regularly inflected, the possessive is formed by adding a morphological zero. The children did not add an additional /-əz/ to these forms, and in the case of **nizzes'*, they erred on the side of removing the plural *-es*, e.g. for the plural possessive they said simply **niz* in those cases where they gave the wrong answers.

It was the adults who had difficulty with the plural possessives: 33 % of them said **wugses* /-zez/ and **bikses* /-sez/, although none said **nizeses* /-əzəz/. This is undoubtedly by analogy with proper nouns in the adults' vocabulary, i.e., no adult would say that if two dogs own hats, they are the **dogses* /-zəz/ hats. However

an adult may know a family named *Lyons*, and also a family named *Lyon*. In the first instance, the family are the *Lyonses* /-zəz/ and if they own a house, it is the *Lyonses'* /-zəz/ house; in the second instance, the family are the *Lyons* and their house is the *Lyons'* /-nz/. The confusion resulting from competing forms like these is such that some speakers do not make this distinction, and simply add nothing to a proper noun ending in /-s/ or /-z/ in order to form the possessive—they say "it is Charles' /-lz/ hat". Some speakers seem also to have been taught in school that they must use this latter form. It seems likely that the children interviewed had not enough grasp of the /-əz/ form for these niceties to affect them.

TABLE 5. PERCENTAGES OF CHILDREN SUPPLYING CORRECT POSSESSIVE FORMS

Singular	*Allomorph*	*Percentage Correct*
wug's	/-z/	84
bik's	/-s/	87
niz's	/-əz/	49
Plural		
wugs'	/-ø/	88
biks'	/-ø/	93
nizzes'	/-ø/	76

ADJECTIVAL INFLEXION

The last of the inflexional items involved attempting to elicit comparative and superlative endings for the adjective **quirky*. The child was shown dogs that were increasingly **quirky* and expected to say that the second was **quirkier* than the first, and that the third was the **quirkiest*. No statistical count was necessary here since of the 80 children shown this picture, only one answered with these forms. Adults were unanimous in their answers. Children either said they did not know, or they repeated the experimenter's word, and said "**quirky*, too". If the child failed to answer, the experimenter supplied the form **quirkier*, and said "This dog is *quirky*. This dog is *quirkier*. And this dog is the ——?" Under these conditions 35 % of the children could supply the *-est* form.

DERIVATION AND COMPOUNDING

The children were also asked several questions that called for

compounding or deriving new words. They were asked what they would call a man who **zibbed* for a living, what they would call a very tiny **wug*, what they would call a house a **wug* lives in, and what kind of dog a dog covered with **quirks* is.

Adults unanimously said that a man who **zibs* is a **zibber*, using the common agentive pattern *-er*. Only 11 % of the children said **zibber*. 35 % gave no answer. 11 % said **zíbbingmàn* and 5 % said **zíbmàn*, compounds that adults did not utilize. The rest of the children's answers were real words like *clown* or *acrobat*.

For the diminutive of **wug*, 50 % of the adults said **wuglet*. Others offered *líttle *wùg*, **wuggie*, **wugette*, and **wugling*. No child used a diminutive suffix. 52 % of the children formed compounds like *bábу *wùg*, *teény *wùg*, and *líttle *wùg*. Two children, moreover, said a little **wug* is a **wig*, employing sound symbolism—a narrower vowel to stand for a smaller animal. For the house a **wug* lives in, 58 % of the adults formed the asyntactic compound **wúghoùse*. Others said **wuggery*, **wúgshoùse*, and **wúghùt*. Again, no child used a suffix. The younger children did not understand this question, and where the older children did, they formed compounds. 18 % of the first graders said **wughoùse*. Others suggested *birdcage* and similar forms. What emerges from this picture is the fact that whereas adults may derive new words, children at this stage use almost exclusively a compounding pattern, and have the stress pattern " at their disposal: the adults unanimously said that a dog covered with **quirks* is a **quirky* dog. 64 % of the children formed the compound **quírk dòg* for this item, and again, no child used a derivational suffix.

Analysis of Compound Words

After the child had been asked all of these questions calling for the manipulation of new forms, he was asked about some of the compound words in his own vocabulary; the object of this questioning was to see if children at this age are aware of the separate morphemes in compound words. The children's explanations fall roughly into four categories. The first is identity: "a blackboard is called a *blackboard* because it is a blackboard." The second is a statement of the object's salient function or feature: "a blackboard is called a *blackboard* because you write on it." In the third type of explanation, the salient feature

happens to coincide with part of the name: "a blackboard is called a *blackboard* because it is black;" "a merry-go-round is called a *merry-go-round* because it goes round and round". Finally, there is the etymological explanation given by adults—it takes into account both parts of the word, and is not necessarily connected with some salient or functional feature: "Thanksgiving is called *Thanksgiving* because the pilgrims gave thanks."

Of the children's answers, only 13 % could be considered etymological. Most of their answers fell into the salient-feature category, while the number of identity responses dropped from the younger to the older group. Many younger children offered no answers at all; of the answers given, 23 % were identity. Of the older children, only 9 % gave identity answers, a difference that was significant at the 1 % level.

As we might expect, the greatest number of etymological responses—23 %—was given for *Thanksgiving*, which is an item that children are explicitly taught. It must be noted, however, that despite this teaching, for 67 % of the children answering this item, Thanksgiving is called *Thanksgiving* because you eat lots of turkey.

The salient feature answers at first seem to have the nature of an etymological explanation, in those instances where the feature coincides with part of the name—72 % of the answers, for instance, said that a fireplace is called a fireplace because you put fire in it. When the salient feature does not coincide with part of the name, however, the etymological aspects also drop out. For *birthday*, where to the child neither the fact that it is a day nor that it is tied to one's birth is important, the number of functional answers rises: it is called *birthday* because you get presents or eat cake. Only 2 % said anything about its being a day.

The child approaches the etymological view of compound word through those words where the most important thing about the word so far as the child is concerned coincides with part of the name. The outstanding feature of a merry-go-round is that it does, indeed, go round and round, and it is the eminent appropriateness of such names that leads to the expectation of meaningfulness in other compound words.

Although the number of etymological explanations offered by the children was not great, it was clear that many children have what amounts to private meanings for many compound words. These meanings may be unrelated to the word's history, and

unshared by other speakers. Examples of this can be seen in the following.

"An airplane is called an *airplane* because it is a plain thing that goes in the air."

"Breakfast is called *breakfast* because you have to eat it fast when you rush to school."

"Thanksgiving is called that because people give things to one another." (Thingsgiving?)

"Friday is a day when you have fried fish."

"A handkerchief is a thing you hold in your hand, and you go 'kerchoo'."

These examples suffice to give the general nature of the private meanings children may have about the words in their vocabulary. What is of additional interest, is that the last explanation about the handkerchief was also offered by one of the college-graduate adult subjects.

We must all learn to handle English inflexion and some of the patterns for derivation and compounding. So long as we use a compound word correctly, we can assign any meaning we like to its constituent elements.

Conclusion

In this experiment, preschool and first grade children, ranging from four to seven years in age, were presented with a number of nonsense words and asked to supply English plurals, verb tenses, possessives, derivations and compounds of those words. Our first and most general question had been: do children possess morphological rules? A previous study of the actual vocabulary of first graders showed that they know real items representing basic English morphological processes. Asking questions about real words, however, might be tapping a process no more abstract than rote memory. We could be sure that our nonsense words were new words to the child, and that if he supplied the right morphological item he knew something more than the individual words in his vocabulary: he had rules of extension that enabled him to deal with new words. Every child interviewed understood what was being asked of him. If knowledge of English consisted of no more than the storing up of many memorized words, the child might be expected to refuse to answer our questions on the grounds that he had never before heard of a **wug*, for instance, and could not possibly give us the plural form since no one

had ever told him what it was. This was decidedly not the case. The children answered the questions; in some instances they pronounced the inflexional endings they had added with exaggerated care, so that it was obvious that they understood the problem and wanted no mistake made about their solution. Sometimes, they said "That's a hard one," and pondered a while before answering, or answered with one form and then corrected themselves. The answers were not always right so far as English is concerned; but they were consistent and orderly answers, and they demonstrated that there can be no doubt that children in this age range operate with clearly delimited morphological rules.

Our second finding was that boys and girls did equally well on these items. Sometimes the girls had a higher percentage of right answers on an item, and more often the boys did somewhat better, but no pattern of differences could be distinguished and the differences were never statistically significant. These findings are at variance with the results of most other language tests. Usually, girls have been shown to have a slight advantage over boys. In our experiment, girls were no more advanced than boys in their acquisition of English morphology. Since other language tests have not investigated morphology *per se*, it is easy enough to say that this is simply one area in which there are no sex differences. A reason for this lack of difference does, however, suggest itself: and that is the very basic nature of morphology. Throughout childhood, girls are perhaps from a maturational point of view slightly ahead of the boys who are their chronological age mates. But the language differences that have been observed may be culturally induced, and they may be fairly superficial. Some social factor may lead girls to be more facile with words, to use longer sentences, and to talk more. This can be misleading. A girl in an intellectual adult environment may, for instance, acquire a rather sophisticated vocabulary at an early age. This should not be taken to mean that she will learn the minor rules for the formation of the plural before she learns the major ones, or that she will necessarily be precocious in her acquisition of those rules. What is suggested here is that every child is in contact with a sufficiently varied sample of spoken English in order for him to be exposed at an early age to the basic morphological processes. These processes occur in simple sentences as well as in complex ones. Practice with a limited vocabulary may be as effective as practice with an extensive vocabulary, and the

factors that influence other aspects of language development may have no effect on morphological acquisition. Since, moreover, this type of inner patterning is clearly a cognitive process, we might expect it to be related to intelligence more than to any other feature. Unfortunately, there were no IQs available for the subjects, so that a comparison could not be made, and this last must remain a speculation.

Our next observation was that there were some differences between the preschoolers and the first graders. These were predominantly on the items that the group as a whole did best and worst on: since no child in the preschool could supply the irregular past *rang*, and a few in the first grade could, this difference was significant. Otherwise, the improvement was in the direction of perfecting knowledge they already had—the simple plurals and possessives, and the progressive tense. The answers of the two groups were not qualitatively different: they both employed the same simplified morphological rules. Since this was true, the answers of both groups were combined for the purpose of further analysis.

Children were able to form the plurals requiring /-s/ or /-z/, and they did best on the items where general English phonology determined which of these allomorphs is required. Although they have in their vocabularies real words that form their plural in /-əz/, in the age range that was interviewed they did not generalize to form new words in /-əz/. Their rule seems to be to add /-s/ or /-z/, unless the word ends in /s z š ž č ǰ/. To words ending in these sounds they add nothing to make the plural—and when asked to form a plural, repeat the stem as if it were already in the plural. This simplification eliminates the least common of the productive allomorphs. We may now ask about the relative status of the remaining allomorphs /-s/ and /-z/. For the items like **lun* or **cra*, where both of these sounds could produce a phonologically possible English word, but not a plural, no child employed the voiceless alternant /-s/. This is the second least common of the three allomorphs. The only places where this variant occurred were where the speaker of English could not say otherwise. So far as general English phonology is concerned a /-z/ cannot in the same cluster follow a /-k-/ or other voiceless sound. Once the /-k-/ has been said, even if the speaker intended to say /-z/, it would automatically devoice to /-s/. The only morphological rule the child is left with, is the addition of the /-z/ allomorph, which is the most extensive: the /-əz/ form for him

is not yet productive, and the /-s/ form can be subsumed under a more general phonological rule.

What we are saying here is that the child's rule for the formation of the plural seems to be: "a final sibilant makes a word plural". The question that arises is, should we not rather say that the child's rule is: "a voiceless sibilant after a voiceless consonant and a voiced sibilant after all other sounds makes a word plural." This latter describes what the child actually does. However, our rule will cover the facts if it is coupled with a prior phonological rule about possible final sound sequences. The choice of the voiceless or voiced variant can generally be subsumed under phonological rules about final sound sequences; the exceptions are after vowels, semivowels, and /l- n- r-/. In these places where phonology leaves a choice, /-z/ is used, and so the child's conscious rule might be to add /-z/. It would be interesting to find out what the child thinks he is saying—if we could in some way ask him the general question, "how do you make the plural?"

Another point of phonology was illustrated by the children's treatment of the forms **heaf* and **kazh*. It was demonstrated here that the children have phonological rules, and the direction of their generalizations was dictated by English phonology, and not simple phonetic similarity. /-ž/ is a comparatively rare phoneme, and yet they apparently recognized it as belonging to the sibilant series in English, and they rarely attempted to follow it with another sibilant. The similarity between /f/ and the sibilants, did not, on the contrary cause them to treat it as a member of this class. The final thing to be noted about **heaf* is that several children and many adults said the plural was **heaves*. This may be by analogy with *leaf: leaves*. If our speculation that the /-z/ form is the real morphological plural is right, there may be cases where instead of becoming devoiced itself, it causes regressive assimilation of the final voiceless consonant.

The allomorphs of the third person singular of the verb and the possessives of the noun are the same as for the noun plural, except that the plural possessives have an additional zero allomorph. These forms were treated in the same way by the children, with one notable exception: they were more successful in adding the /-əz/ to form possessives and verbs than they were in forming noun plurals. They were asked to produce three nearly identical forms: a man who **nazzes;* two **nizzes;* and a **niz's* hat. On the verb they were 48 % right; on the possessive they were 49 % right, and on the noun plural they were only 28 % right. The

difference between their performance on the noun plural and on the other two items was significant at the 1 % level. And yet the phonological problem presented by these three forms was the same. For some reason the contingent rule for the formation of the third person singular of the verb and for the possessive is better learned or earlier learned than the same rule for the formation of noun plurals. The morphological rule implies meaning, and forms that are phonologically identical may be learned at different times if they serve different functions. These forms are not simply the same phonological rule, since their different functions change the percentage of right answers. Perhaps the child does better because he knows more verbs than nouns ending in /s z š ž č ǰ/, and it is possible that he has heard more possessives than noun plurals. It is also possible that for English the noun plural is the least important or most redundant of these inflexions. This is a somewhat surprising conclusion, since nouns must always appear in a singular or plural form and there are ways of avoiding the possessive inflexion: it is generally possible to use an *of* construction in place of a possessive—we can say *the leg of the chair* or *the chair's leg*, or *the chair leg* although in cases involving actual ownership we do not say *of*. A sentence referring to *the hat of John* sounds like an awkward translation from the French. And no child said it was *the hat of the *niz*. The children's facility with these forms seems to indicate that the possessive inflexion is by no means dying out in English.

Of the verb forms, the best performance was with the present progressive: 90 % of all the children said that a man who knew how to **zib* was **zibbing*. Undoubtedly, children's speech is mostly in the present tense, and this is a very commonly-heard form. Explanations of what is happening in the present all take this form. "The man is *running*" — or *walking* or *eating* or *doing* something. The additional point is that the *-ing* forms are not only very important; this inflexion has only one allomorph. The rules for its application are completely regular, and it is the most general and regular rules that children prefer.

The children's handling of the past tense parallels their treatment of the plurals, except that they did better on the whole with the plurals. Again, they could not extend the contingent rule. Although they have forms like *melted* in their vocabulary, they were unable to extend the /-əd/ form to new verbs ending in /t d/. They treated these forms as if they were already in the past. They applied the allomorphs /-d/ and /-t/ appropriately where they

were phonologically conditioned, and only /-d/ to a form like **spow*, where either was possible. This suggests that their real morphological rule for the formation of the past is to add /-d/, and under certain conditions it will automatically become /-t/. Many adult speakers feel that they are adding a /-d/ in a word like *stopped;* this may be because of the orthography, and it may be because they are adding a psychological /-d/ that devoices without their noticing it.

Whereas the children all used regular patterns in forming the past tense, we found that for adults strong pasts of the form *rang* and *clung* are productive. Since virtually all English verbs that are in the present of an *-ing* form make their pasts irregularly, this seemed a likely supposition. Adults made **gling* and **bing* into **glang* and **bang* in the past. New words of this general shape may therefore be expected to have a very good chance of being treated according to this pattern — real words like the verb *to string* for instance, have been known the vacillate between the common productive past and this strong subgroup and finally come to be treated according to the less common pattern. The children, however, could not be expected to use this pattern since we could not demonstrate that they had the real form *rang* in their repertory. They said **ringed.* At one point, the experimenter misread the card and told the child that the bell *rang.* When the child was asked what the bell did, he said, "It **ringed.*" The experimenter then corrected him and said, "You mean it *rang.*" The child said that was what he had said, and when asked again what that was, he repeated, "It *ringed,*" as if he had not even heard the difference between these two allomorphs. Perhaps he did not.

The adults did not form irregular pasts with any other pattern, although a form was included that could have been treated according to a less common model. This was the verb **mol*, which was of the pattern *cut* or *bet.* There are some 19 verbs in English that form their past with a zero morpheme, but this group does not seem to be productive.

The cases of **gling*, which became **glang* in the past and **mol*, which became **molled* suggest some correlates of linguistic productivity. About nineteen verbs in English form their past tense with a zero allomorph. About 14 verbs form their past like *cling*, and seven follow the pattern of *ring.* Within these last two groups there are words like *win*, which becomes *won* and *swim*, which becomes *swam.* We can also find words similar to

win and *swim* that are quite regular in the past: *pin* and *trim*. But virtually all of the verbs that end in *-ing* form their past in *-ang* or *-ung*. There are approximately 10 of these *-ing* verbs.

The productivity of the *-ang* and *-ung* forms proves that new forms are not necessarily assimilated to the largest productive class. Where a small group of common words exist as a category by virtue of their great phonetic similarity and their morphological consistency, a new word having the same degree of phonetic similarity may be treated according to this special rule. *Ox* : *oxen* is not similarly productive, but probably would be if there were just one other form like *box* : *boxen*, and the competing *fox* : *foxes* did not exist. With **mot*, the zero allomorph is not productive because although it applies to more cases than are covered by the *-ing* verbs, it is not so good a rule in the sense that it is not so consistent. The final /-t/, which is the only common phonetic element, does not invariably lead to a zero allomorph, as witness *pit* : *pitted*, *pat* : *patted*, and many others.

Although the adults were uniform in their application of *-er* and *-est* to form the comparative and superlative of the adjective, children did not seem to have these patterns under control unless they were given both the adjective and the comparative form. With this information, some of them could supply the superlative.

Derivation is likewise a process little used by children at this period when the derivational endings would compete with the inflexional suffixes they are in the process of acquiring. Instead, they compound words, using the primary and tertiary accent pattern commonly found in words like *blackboard*.

The last part of the experiment was designed to see if the children were aware of the separate elements in the compound words in their vocabulary. Most of these children were at the stage where they explained an object's name by stating its major function or salient feature: a blackboard is called a *blackboard* because you write on it. In the older group, a few children had noticed the separate parts of the compound words and assigned to them meanings that were not necessarily connected with the word's etymology or with the meaning the morphemes may have in later life. Not many adults feel that Friday is the day for frying things, yet a number admit to having thought so as children.

These last considerations were, however, tangential to the main problem of investigating the child's grasp of English morphological rules and describing the evolution of those rules. The picture that emerged was one of consistency, regularity, and simplicity. The

children did not treat new words according to idiosyncratic pattern. They did not model new words on patterns that appear infrequently. Where they provided inflexional endings, their best performance was with those forms that are the most regular and have the fewest variants. With the morphemes that have several allomorphs, they could handle forms calling for the most common of those allomorphs long before they could deal with allomorphs that appear in a limited distribution range.

Massachusetts Institute of Technology.

FUSED UNITS IN PROSODIC ANALYSIS

PAUL L. GARVIN and MADELEINE MATHIOT

0. The thesis of this paper is that an integrational approach to the prosodic aspect of phonemic problems will yield both increased analytic efficiency and greater interpretive power as over a primarily linear treatment.

By a linear approach we mean an investigation of phonemic patterns based on the assumption that segmental phonemes constitute sequences, with certain special phonemes—called junctural—forming anchorages of the sequences, and with other special phonemes (such as stress and pitch)—called suprasegmental—accompanying the linear segmental phonemes in additional, coterminous sequences.

Hockett's discussion of "microsegments" and "macrosegments" has been a significant move in another direction of analysis.[1] Pike's chapter on "Higher-layered units of the manifestation mode of the uttereme"[2] is an attempt to develop the theory of the integrational approach. In spite of his cumbersome terminology (e. g. "hyperphonemes" for phonemic fused units), we find ourselves in basic agreement with his point of view.

In the present paper, we want to take an operational rather than theoretical approach and present a phonemic analysis based on the integrative rather than the linear assumption.

The integrational approach posits that segmental phonemes do not simply constitute sequences, but that they fuse into units of higher orders, just as morphemes fuse into words or other higher-order units in morphemics. These higher-order phonemic units

[1] Charles F. Hockett, "Peiping Phonology," *Journal of the American Oriental Society* 67.253-67 (1947); also id., *A Manual of Phonology* (Baltimore, 1955), p. 43: "Ultimate phonologic constituents do not occur in an utterance as the individual bricks occur in a row of bricks. Rather, they occur in clusterings, these occur in still larger clusterings, and so on, up to the level of the whole utterance."

[2] Kenneth L. Pike, *Language in Relation to a Unified Theory of Human Behavior*, Part II, Prelim. Ed. (Clendale, Calif., 1955), pp. 41-70.

are assumed to have, as overall characteristics, (a) the prosodic features which in the linear approach are said to constitute the suprasegmental phonemes, and (b) the boundary features which in the linear approach are said to constitute the juncture phonemes. Finally, both the prosodic and the boundary features are assumed to be systematically related to each other, in terms to be established by the analysis.[3]

On the basis of the above, emphasis in phonemic procedure will be shifted to the following two operations:

(1) the establishment of fused-unit boundaries to obtain proper frames for distributional statements;

(2) the computation, within the spans of fused units of different orders, of the maximum predictability of phonetic detail from a minimum of features assumed to be phonemic. This is particularly relevant in the determination of prosodic features.

Thus, the units and features serving as the best minimum base for maximum predictability will be said to enter into the phonemic pattern.

The description which we are presenting is that of Chamorro, a language of the Philippine branch of the Malayo-Polynesian family, spoken on the island of Guam.[4] The data, consisting of isolated examples and narrative monologue, were recorded on tape and analyzed on the basis of consistent auditory impressions of phonetic oppositions.[5]

1.0. We shall first deal with the consistent phonetic oppositions leading to the establishment of fused-unit boundaries, viz. degrees

[3] For a discussion of phonemic predictability, see Noam Chomsky, Morris Halle, and Fred Lukoff, "On Accent and Juncture in English," *For Roman Jakobson* (The Hague, 1956), pp. 65-80.

[4] An earlier, more detailed version of the phonemic analysis of Chamorro was submitted by Madeleine Mathiot in partial fulfilment of the requirements for the degree of Master of Science in Linguistics at Georgetown University (Washington, 1955).

[5] We agree with statement of the Czech phonemicist František Daneš about the use of instrumental phonetics in phonemics: "Today, many linguists and phoneticians, in spite of the latest improvements, look more soberly and critically upon the possibilities of instrumental investigation, especially in regard to connected discourse.

"There are two flaws in this method; one is of a more general character, the second concerns mainly the investigation of intonation. It is true that instruments are more accurate and sensitive than the human ear..., but instrumental records and their interpretation in terms of physical acoustics do not give us a true picture of the way in which the speakers hear and understand (evaluate) their own language....

"The insufficiency of the instrumental method shows up particularly in the inves-

of pause, and with the phonetic oppositions on the basis of which the phonemic prosodic features of the fused units were established, viz. different degrees of loudness and pitch.

1.1. Two degrees of pause were observed in the connected discourse: a more conspicuous longer pause (length varying from less than a second to more than a second), and a less conspicuous shorter pause (always shorter than the former). These two degrees of pause were defined to be the basic phonetic properties of TERMINAL JUNCTURE and MEDIAL JUNCTURE, respectively. Segments bounded by terminal junctures were defined as PHONEMIC PHRASES; those bound by medial juncture were defined as PHONEMIC CONTOURS.

1.2. Several degrees of loudness were observed, but upon closer examination it was found that in addition to differentially stressed nuclei within contours, contours as wholes differed in overall loudness, with this differential distributed equally over the nuclei, regardless of the degree of stress of the latter. Thus, instead of positing a larger number of degrees of stress, we attributed this contour-length loudness differential to a separate phonemic feature of CONTOUR INTENSITY (classifying contours as strong versus weak), and assigned a phonemic feature of STRESS only to differences in loudness between nuclei within the same contour.[6]

The relation of contour intensity to stress is such that an unstressed nucleus of a weak contour is less loud than an unstressed nucleus of a strong contour, a (primarily or secondarily) stressed

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tigation of pitch and stress and the phonetics of connected discourse in general. The instruments which we have are relatively imperfect, the investigation is cumbersome and complex and can therefore cover only a limited amount of data.... It is not completely true, either, that the instrumental method is free of accident—on the contrary, considering the relative scarcity of records ... the danger of accident is considerably greater than with the auditory method which has almost unlimited data at its disposal....

"The most complete instrumental record becomes useful to linguistics only after analysis and evaluation from the standpoint of the function and significance of the various components in terms of the system of the language. The instrumental record, however, does not by itself allow us to ascertain the significance and function of the various waves, formants, etc.—these must first be discovered, albeit only in outline, by an auditory analysis of spoken language..." (*Intonace a věta ve spisovné češtině*, Prague, 1957, pp. 39-41).

[6] This interpretation was influenced by Uriel Weinreich's *contrastive stress*, which he defines as "facultative relative loudness, actualized only if the construction as a whole is emphasized in the sentence" ("Stress and Word Structure in Yiddish," *The Field of Yiddish*, New York, 1954, p. 3).

nucleus of a weak contour is less loud than a correspondingly stressed nucleus of a strong contour.

1.3. Four phonetic ranges of pitch were observed (from lowest 1 to highest 4). These were found to constitute regular intonation lines within each contour in terms of the vocalic nuclei contained, and the final pitch of the contour was found to be associated with contour intensity as well as with the terminal or medial juncture ending it. In terms of the association of various pitch ranges with the longer pause, three types of terminal juncture (PARAGRAPH-FINAL, HESITATION, and TRANSITION juncture)[7] could be established (see § 2) as against a single type of medial juncture.

1.4. Of these phonemic features, terminal junctures were considered boundary features of the phonemic phrase, medial juncture and contour intensity were considered features of the contour, and stress a feature of the vocalic nucleus.

By resolving the different phonetic degrees of loudness into the separate phonemic oppositions of stress (presence vs. absence of stress) and contour intensity (strong vs. weak), we were able to establish contour intensity, together with a differentiation of the junctures, as the conditioning factors for the predictable occurrence of pitch.

2.0. We shall now discuss in some detail the conditions for pitch predictability.

As was stated in 1.3 above, the final pitch of each contour is predictable in terms of the intensity of the contour and in terms of the following juncture, while the remaining pitches are predictable in terms of the number of syllables (which is the same as the number of nuclei, the syllable boundaries being predictable in terms of the configuration of vowels and consonants).

We shall therefore deal with final and non-final pitches separately and in that order.

2.1. The primary determinant of final pitch is the type of juncture, since it determines the relative pitch range, whereas contour intensity determines the variation within it.

2.1.1. Paragraph-final juncture (symbol: ‡) entails pitch 1 on the final syllable of weak contours, pitch 2 on the final syllable

[7] These terms were selected on the basis of an impression of the semantic function of the junctures in the utterance.

of strong contours (symbols: ˈˈ marks following strong contour, *lp* indicates long pause):

[- - - -¹ *lp*] = / - - - -ǂ/
[ˈˈ - - - -¹ *lp*] = / ˈˈ - - - -ǂ/

Examples: [pinikáktu¹ *lp*] = /pinikáktu ǂ/ 'my steps', versus [ˈˈ mani?lú·hu¹ *lp*] = /ˈˈ mani?lú.hu ǂ/ 'my siblings'.

2.1.2. Hesitation juncture (symbol: .|.) entails pitch 2 on the final syllable of weak contours; it has not been found after strong contours (note that pitch 2 on the final syllable of strong contours is a phonetic characteristic of paragraph-final juncture):

[- - - - ² *lp*] = / - - - - .|./
[ˈˈ - - - - ² *lp*] = / ˈˈ - - - -ǂ/

Examples: [para?í² *lp*] = /para?í .|./ 'for the...', versus [ˈˈ gólay² *lp*] = /ˈˈ gólaiǂ/ 'vegetable'.

2.1.3. Transition juncture (symbol: |) entails pitch 3 on the final syllable of weak contours, pitch 4 on the final syllable of strong contours:

[- - - - ³ *lp*] = / - - - - |/
[ˈˈ - - - - ⁴ *lp*] = / ˈˈ - - - - |/

Examples: [para?ilæncu³ *lp*] = /para?ilæncu | / 'to the ranch', versus [ˈˈ gini?ilæncu⁴ *lp*] = /ˈˈ gini?ilæncu | / 'towards the ranch'.

2.1.4. Medial juncture (symbol: +) entails pitch 2 on the only syllable of monosyllabic weak contours, pitch 3 on the only or final syllable of all other contours (and for all contours a slight lengthening of the final phoneme; symbol *sp* indicates short pause):

[-² *sp*] = / - +/
[ˈˈ -³ *sp*] = / ˈˈ - +/
[- - - -³ *sp*] = / - - - - +/
[ˈˈ - - - -³ *sp*] = / ˈˈ - - - -+/

Examples: [sæ?¹ *sp*] = /sæ?+/ 'because...', versus [ˈˈ sæ?³ *sp*] = /ˈˈ sæ?+/; [zanmátuhu?³ *sp*] = /zanmátuhu?+/ 'when I get to...', versus [ˈˈ za?ù?áti³ *sp*] = /ˈˈ za?ù?áti+/ 'and he/she pours'.

Note again that while [-² *sp*] equals medial juncture, [-² *lp*] equals hesitation juncture (see 2.1.2.).

2.2. The pitch pattern of the non-final syllables of a contour is as follows:

for dissyllabic contours—pitch 2 on the penultimate;

for trisyllabic contours—pitch 1 on the penult, pitch 2 on the antepenult;

for contours of four or more syllables — pitch 1 on the first syllable, pitch 2 on the second syllable, and pitch 1 on every subsequent syllable up to and including the penult.

Examples: (all showing transition juncture): /pá·n|/ = [pá·n^{3}] 'bread', /ta·ta|/ = [tá·^{2}ta^{3}] 'father', /bæ·bali|/ = [bæ·^{2}ba^{1}li^{3}] 'eyelashes', /camó·ruzu$^{?}$|/ = [ca^{1}mó·^{2}ru^{1}zu$^{?3}$] 'I am a Chamorro', /palàu$^{?}$anmá·mi|/ = [pa^{2}làw$^{1?}$an^{1}má·^{3}mi^{1}] 'our (pl.) women'.

3. The detailed relations between contour intensity and stress on the basis of different degrees of loudness were established by comparing syllables of different length and loudness, but each carrying primary stress within the contour, with a selected syllable type serving as a norm.

The loudness of a primarily stressed long syllable of a weak contour—a loudness not affected by the following juncture—was chosen for this norm. In terms of it, the various phonetic loudnesses were interpreted phonemically as follows:

(a) before terminal junctures,
norm loudness — weak contour (short or long stressed vowel)
louder than norm — strong contour (short or long stressed vowel)

(b) before medial juncture,
less loud than norm—weak contour (short stressed vowel)
norm loudness—weak contour (long stressed vowel)
—strong contour (short stressed vowel)
louder than norm—strong contour (long stressed vowel).

Note that before transition juncture,

(a) weak contours with short stressed vowels are consistently somewhat louder than they are before medial juncture;

(b) all strong contours are consistently somewhat louder than they are before medial juncture.

Examples (phonetic symbols: ′ norm loudness, ˋ below-norm loudness, ˝ above-norm loudness): [$^{?}$átti *lp*] = /$^{?}$átti ‡/ 'tricks', [$^{?}$itimóña *lp*] = /˝$^{?}$itimóña ‡/ 'his/her knees', [$^{?}$afacicu$^{?}$u *sp*] = /$^{?}$afacicu$^{?}$u+/ 'my work', [zamá·naw *sp*] = /zamá·nau+/ 'and I went', [$^{?}$untúŋu *sp*] = /˝$^{?}$untúŋu+/ 'you (sg.) know', [$^{?}$uhucú· *sp*] = /˝$^{?}$uhucú·+/ 'I close(d)'.

4.0. Since of the two orders of phonemic fused units the contour is that which, though bounded by junctures, does not itself contain a juncture, it was selected as the frame for describing vowel and consonant distribution.

Within the contour, three phonemic degrees of stress could be discerned (primary, secondary, unstressed), as well as phonemic vowel length under primary stress in open syllables only.

The distributional analysis also revealed two additional predictabilities of phonetic features: vowel syllabicity, and points of syllable division.

4.1. Vowel inventories under different conditions of stress are as follows:

Under primary stress

i, i·	u, u·
e, e·	o, o·
æ, æ·	a, a·

Under secondary stress

i	u
e	o
a	

In unstressed position

i	u
a	

The primarily stressed position thus emerges as that of maximum differentiation, the unstressed position as that of minimum differentiation.

4.2. The consonant inventory is as follows:

stops	p, t, k, ʔ
	b, d, g
affricates	c, z
spirants	f, s, h
nasals	m, n, ñ, ŋ
liquids	l, r

Clusters of two and three consonants were found, the latter only with liquid as the final member.

4.3. Unstressed high vowels are non-syllabic under the following conditions:

(a) both before and after non-identical (primarily or secondarily) stressed vowel, and only before identical secondarily stressed

vowel (after identical secondarily stressed vowel, the unstressed vowel remains syllabic);

(b) adjacent to unstressed /a/;

(c) of two unstressed high vowels adjacent to each other, the first is non-syllabic;

(d) between any two other vowels, an unstressed high vowel is non-syllabic;

(e) of two unstressed high vowels enclosing stressed or unstressed /a/, the second is consistently non-syllabic and the first is optionally non-syllabic.

Conditions (a)——(c) give rise to the following phonetic diphthongs:

Table for conditions (a)

	[y-]	[-y]	[w]	[-w]
i			[-wi-] = /-ui-/	
			[-wì-] = /-uì-/	
u	[-yú-] = /-iú-/			
e	[-yé-] = /-ié-/	[-éy-] = /-éi-/	[-wé-] = /-ué-/	
		[-èy-] = /-èi-/	[-wè-] = /-uè-/	
o	[-yó-] = /-ió-/			
æ	[-yæ-] = /-iæ-/	[-æy-] = /-æi-/	[-wæ-] = /-uæ-/	[-æw-] = /-uæ-/
a	[-yá-] = /-iá-/	[-áy-] = /-ái-/	[-wá-] = /-uá-/	[-áw-] = /-áu-/
	[-yà-] = /-ià-/	[-ày-] = /-ài-/		[-àw-] = /-àu-/

Table for conditions (b)

	[y-]	[-y]	[w-]	[w-]
a	[-ya-] = /-ia-/	[-ay-] = /-ai-/	[-wa-] = /-ua-/	[-aw-] = /-au-/

Table for conditions (c)

	[y-]	[w-]
i	[-yi-] = /-ii-/	[-wi-] = /-ui-/
u	[-yu-] = /-iu-/	[-wu-] = /-uu-/

4.4. Points of syllable division were investigated by observing the informant's consistent syllabicizing. These points regularly fall:

(a) before an intervocalic single consonant or (phonetically) non-syllabic vowel;

(b) between the two consonants of a two-consonant cluster, or between the first two consonants of a three-consonant cluster.

5. We now want to relate our analytic results to our initial assumption that fused units are structural units and not merely analytic constructs, by showing that they exhibit certain properties assignable only to them as wholes and not derivable from the

sum of their components. These we call FUSIONAL PROPERTIES.

Two classes of fusional properties can be assigned to Chamorro fused units:

(a) Prosodic characteristics shared by all the components of a given fused unit — SHARED PROPERTIES;

(b) Prosodic characteristics not shared by all the components of a fused unit, but differentially statable only in terms of the fused unit as a frame — DIFFERENTIAL PROPERTIES.

These two types of fusional properties are related to each other in that a shared property of a lower-order fused unit constitutes a differential property of the unit of the next higher order.

In the above terms, Chamorro contours can be assigned four fusional properties: one phonemic shared property (contour intensity), two phonemic differential properties (stress and length), and one non-phonemic differential property (pitch). Chamorro phonemic phrases can be assigned only one fusional property, the phonemic differential property of contour intensity. Contours thus have many more fusional properties than phonemic phrases. Assuming that fusional properties account for the integratedness of fused units, different degrees of integratedness can be posited for units of different orders: that of the contour is much higher than that of the phrase. The fused unit with the highest degree of integratedness can be called the TYPICAL FUSED UNIT for the given language; for Chamorro, it is the contour.

Languages then become comparable in terms of their typical fused units and in terms of the number and kind of the fusional properties of the latter. Within a given language, different speech styles become comparable in terms of the statistics of the typical fused unit—in Chamorro, contour length and number of contours per phrase. The style studied here (narrative monologue) is characterized by short contours (averaging 3-4 syllables) and most commonly two contours per phrase.

The typical fused unit may finally serve as the analytic correlate of such impressionistic observations as the overall acoustic impression of "what a language sounds like" or the melodic and rhythmical impressions of a particular "foreign accent." Thus, the "choppy" impression of the imperfect English of Chamorro speakers can be accounted for by the segmentation into short contours and the prominence of contour-final pitches carried over from Chamorro.

Institute of Languages and Linguistics
Georgetown University.

RECOGNITION OF WORD TONES IN WHISPERED SPEECH

MARTIN KLOSTER JENSEN

The present paper deals with the question of whether, and how, phonemically distinctive tone is rendered in whispered speech. Since it is possible that there may be differences between languages in the manner and effectiveness of whispered tone rendition, the study deals with several "tone languages"—Norwegian, Swedish, Slovenian, and Mandarin—rather than with a single language.

We will first summarize the history of the problem.

The question whether voice pitch is heard in whispered Chinese was brought up by Olof Gjerdman in 1924. Gjerdman had asked a Chinese if two persons could understand each other when whispering in Chinese, and he took the affirmative answer to corroborate his theory that "a whispered language has as many distinct musical accents as the same language when voiced."[1] According to Pike,[2] it is possible that stress replaces pitch in whisper, thus enabling one to distinguish "tones." Pater Giet has maintained that the word tones are both expressed and perceived in whispered Chinese: "Auch beim Flüstern spricht und hört der Chinese die Töne".[3] Eugen Dieth stated briefly that it is possible to indicate intonation in whisper to a certain extent ("einigermassen")[4], and tried to account for this by postulating changes in the air pressure. He tried to invalidate a statement by Gutzmann, that "das Flüstergeräusch ist in seiner Höhe an sich nicht variabel" by referring to the fact that whispered Chinese is fully understandable. This, however, is no proof that pitch exists in whisper: context of course always helps one to understand, and the same may be true of

[1] "Critical Remarks on Intonation Research," *School of Oriental Studies, London Institution*, Vol. III, part III, p. 502.

[2] Kenneth L., Pike, *Tone Languages*, Ann Arbor, 1948, p. 34.

[3] Franz Giet, *Zur Tonität nordchinesischer Mundarten* (Studia Instituti Anthropos), Wien/Freiburg 1950, p. 95.

[4] Eugen Dieth, *Vademekum der Phonetik*, Bern, 1950, p. 96.

stress patterns which, for all we know, may make tone distinctions more or less superfluous.

In 1955, W. Meyer-Eppler made sound spectrograms of a sentence which was first spoken with voice and subsequently whispered.[5] On the basis of these spectrograms he concluded that pitch modulation ("Tonhöhenverlauf") is actually found in whisper, though it is brought about by certain substitutes for phonation. At the same time, G. Panconcelli-Calzia showed that a sentence from a well-known opera text "sung" in whisper to listening subjects was invariably heard as just whispered speech, not "song".[6] Panconcelli-Calzia also had Chinese-speaking subjects listen to whispered "word tones" in Chinese. The way this experiment was conducted was criticized by Pater Giet,[7] who vigorously affirmed his standpoint as expressed in his book.[8]

In 1956, Meyer-Eppler reported on fresh experiments which allegedly prove that vowels "sung" in whisper in "the first five tones of a diatonic scale" display a substitute for rise in pitch: added noise in the sound spectra of [e, i, o] and a change in the location of formants in [a, u].[9] Meyer-Eppler reaffirms the validity of the statement made by Giet in *Lingua*, namely that a change in formants (i.e. the character of the vowel) may cause the impression of a change in pitch. This agrees with the well-known phenomenon that in ordinary speech, change in pitch may lead to a change in perceived vowel quality.[10]

In 1957, Meyer-Eppler's experiment with whispered "song" was, in all essentials, duplicated by C. M. Wise and L. P.-H. Chong[11]. They, however, found nothing in the phonetic substance that might indicate differences in "pitch." On the basis of the somewhat tenuous assumption that "the difference between the tones

[5] "Experimentelle Untersuchungen zum Mechanismus von Stimme und Gehör in der lautsprachlichen Kommunikation," *Forschungsberichte des Wirtschafts- und Verkehrsministeriums Nordrhein-Westfalen*, No. 221, 1955, pp. 18-19.

[6] "Das Flüstern in seiner physio-pathologischen und linguistischen Bedeutung," *Lingua*, IV (1954-55), p. 371.

[7] Franz Giet, "Kann man in einer Tonsprache flüstern?" *Lingua*, V (1955-56), pp. 372-381.

[8] See reference in footnote 3.

[9] "Realization of Prosodic Features in Whispered Speech," *Journal of the Acoustical Society of America*, XXIX (1957), pp. 104-106.

[10] Cf. E. A. Meyer, "Zur Tonbewegung des Vokals im gesprochenen und gesungenen Einzelwort," *Phonetische Studien*, 1896-97, p. 20.

[11] C. M. Wise and Lily Pao-Hu Chong, "Intelligibility of Whispering in a Tone Language," *Journal of Speech and Hearing Disorders*, XXII (1957), pp. 335-338.

of a pair of otherwise identical-sounding words in Mandarin is a difference of pitch" (no mention being made of the loudness feature), they concluded that "such words actually could not be distinguished from each other if spoken in a whisper." They accounted for the intelligibility of Chinese in whispered conversation by the role of the context, and used the same explanation for the fact that 62 % of 3824 utterances, with a possible choice among four items for each utterance, were recognized correctly. A choice among four distinctive word tones is probably relatively rare in Chinese, and until we know more about the test material which the authors used, it is difficult to know what 62 % represents as a recognition score. It is encouraging to see from the article in question that the same authors are conducting an experiment with words spoken in isolation. (The attention of the present writer was drawn to the article of Wise and Chong after he had himself carried out experiments with whispered word tones in various languages.)

In my opinion, experimental phonetics cannot be used to demonstrate what is and what is not perceived by the ear; in other words, experimental phonetics is incapable of bringing out all the physical correlates to language stimuli. I am therefore inclined to draw no conclusion from the instrumental experiments of Meyer-Eppler or Wise and Chong as far as perceptibility is concerned. I prefer direct perception tests, which have been regrettably rare in the study of word tones in whisper.

It does not seem very probable that substitutes for pitch stimuli are used in whispered speech, since the employment of such substitutes would demand of speakers a different technique from that which is used normally. A different technique, such as using more stress in lieu of higher pitch, would have to be acquired by observation and imitation. This would be very difficult, since the child learning to speak in whisper can have no clear idea of what the relation is between stress and intended pitch in that particular kind of speech. Pitch substitutes other than stress, e.g. changes in the character of the vowel, are still less directly observable. But it is useful to distinguish between substitutes for pitch and the "phonologization", in the absence of voicing, of features which are present even in voiced speech. It may even be that this is what Meyer-Eppler would call substitutes for pitch characteristics.

In the following, we report some recognition tests for word tones

in whispered speech which were carried out during the author's stay as a guest in the Department of Linguistics of Harvard University, in the spring of 1958. The experiments are listed according to the language involved.

1. Norwegian

a. Subject A. S. (female, Oslo dialect, linguistically trained), was given a minimal pair of words: /sva:re/ with Tone 1, 'the answer', and /sva:re/ with Tone 2, 'to answer' (in Norwegian orthography, *svaret* and *svare*). A coin was thrown; if it showed heads, the subject was to whisper the word with Tone 1; if tails, with Tone 2. This was done 33 times and tape-recorded, while a written record was kept of the throws. The subject then listened back to the tape-recorded 33 words, and indicated for each word the tone she thought she heard. Like all the other subjects, she had been requested to make a choice of tones even if she was in doubt. The whole series of 33 words was played back to the subject three times, and therefore 99 indications of perceived tone were given. Of these 65 % were "correct," i.e. in agreement with the Tone 1 vs. Tone 2 instructions produced by the coin.

b. Same subject, same procedure, but with 50 whispered words, to which she listened back twice, so that 100 indications of perceived tone were given.[12] Here 70 % were in agreement with the record kept of heads and tails in coin-throwing, as explained for experiment 1a.

c. Subject, the author (same dialect as A.S.). Same test as 1b. Recognition score: 61 %.

d. Subject R. M. (female, Stavanger dialect; not linguistically trained, but taught to denote tones as numbers 1 and 2). Same test as 1b. Recognition score: 71 %.

e. Subjects A. S. and M. K. J. (the author). A. S. was given a thoroughly shuffled deck of one hundred cards. The deck contained five instances of each of the following forms, which occur

[12] In order to make it impossible for the subject to remember what he or she had said in the beginning or at the end, the two first and the eight last words out of 60 were cut out. This left us with 50 words for counting the score.

in pairs that are minimal except for tone (the *-t* is purely orthographic):

sivet 'the bulrush'	*sive* '(to) ooze'
svevet 'the floating'	*sveve* '(to) float'
været 'the weather'	*være* '(to) be'
svaret 'the answer'	*svare* '(to) answer'
lånet 'the loan'	*låne* '(to) borrow'
bordet 'the table'	*borde* '(to) board'
huset 'the house'	*huse* '(to) house'
lyset 'the light'	*lyse* '(to) shine'
løvet 'the leafage'	*løve* 'lion'
høyet 'the hay'	*høye* 'high', adj.

The whispering was not tape-recorded, but listened to directly by M. K. J. turning his back on the subject who whispered, so that he should not be helped by gestures such as raising or lowering of eyebrows, bending or stretching of neck, etc. M. K. J. wrote down the words he heard. M. K. J.'s recognition score: 53 %.

f. Same procedure as in 1e, but this time M. K. J. whispered and A. S. listened. A. S.'s recognition score: 73 %.

Summary of Recognition Scores for Norwegian

Subject whispering	Subject listening and recognizing	Experiment	Score
A. S.	A. S.	a	65 %
A. S.	A. S.	b	70 %
M. K. J.	M. K. J.	c	61 %
R. M.	R. M.	d	71 %
A. S.	M. K. J.	e	53 %
M. K. J.	A. S.	f	73 %

As a 50 % score means pure randomness, it will be seen that the above recognition scores are not very convincing. However, they show some degree of distinction. The two poorest scores were obtained by the author (experiments c,e), who, however, had been the whispering subject in the experiment which came out with the highest score (f, 73 %).

2. Swedish.

Subject P. E. T. (male, southern Swedish dialect, linguistically trained). Same procedure as in 1b. Test words: *tagen* ('the repeated pulling', plural) and *tagen* ('taken', past participle), identical except for tone. Recognition score: 100 %.

The subject said he was never in doubt about the "tone" heard. He and the author agreed that what distinguished the pronunciation of the two words was a very marked stress on the second syllable in the case of the past participle. More extensive testing is of course needed before anything can be said about whispered tones in Swedish.

3. Slovenian.

a. Subject R. Z. (male, Ljubljana dialect, linguistically trained). Procedure as in 1b. Test words: *pet* ('five', numeral), tone: "long falling," and *pet* ('to sing'), tone: "long rising." Recognition score: 73 %.

b. Same procedure as 3a, except that the test words were *las* ('strand of hair', nom. sg.), tone: "long falling," and *las* ('of the hair', gen. pl.), tone: "long rising." Recognition score: 77 %.

c. Subject A. G. (male, Maribor dialect, not linguistically trained, but recognizing the tones in voiced speech as numbers 1 and 2). Test words: *rak* ('crab', dat. sg.), tone: 'long falling,", and *rak* ('crabs', nom. pl.), tone: "long rising." Same procedure as in 1b. Recognition score: 85 %.

d. Subject A. G. listening to the tape made in experiment 3a (R. Z. whispering). 100 items for recognition. Score: 74 %.

e. Subject A. G. listening to the tape made in experiment 3b (R. Z. whispering). 100 items for recognition. Score: 71 %.

The subjects pointed out that they had different dialects. They also complained of noise during the playback process, which may have caused lower recognition scores than would have been achieved in the case of better reproduction.

Summary of Recognition Scores for Slovenian

Subject whispering	Subject listening and recognizing	Experiment	Score
R. Z.	R. Z.	a	73 %
R. Z.	R. Z.	b	77 %
A. G.	A. G.	c	85 %
R. Z.	A. G.	d	74 %
R. Z.	A. G.	e	71 %

On inspection, the results seem to be more successful in the Slovenian case than in the Norwegian one. For a possible

explanation, compare the comments following the Table of recognition scores for Chinese, below.

4. Chinese (Mandarin).

a. Subject Mr. K. C. C. (anthropologist). Test material: *lha* ('to pull'), tone 1, and *la* ('to cut'), tone 2. The subject was told before the testing took place that in this experiment there would only be a choice between tones 1 and 2. Same procedure as in experiment 1b.[13] Recognition score: 83 %.

b. Subject Mrs. K. C. C. (anthropologist). Same procedure and test material as in experiment 4a. Recognition score: 79 %.

c. Same procedure as in experiment 1e. The test material consisted 120 whispered words, 12 occurring 10 times each. Randomness in sequence was secured by shuffling cards (see experiment 1e). Of the four word tones in Mandarin, only two were chosen for each minimal set. There were six minimal sets, each representing one tonal contrast: 1/2, 1/3, 1/4, 2/3, 2/4, 3/4. The subjects were informed about the special contrast applied to each of the minimal sets. The 12 set members were:[14]

Minimal set no.	Word	Translation	Tone no.
1	*lha*	to pull	1
	la	to cut	2
2	*ba*	eight	1
	baa	numeral adjunct for a noun	3
3	*la*	he (pron.)	1
	lah	a couch	4
4	*far*	to punish	2
	faa	method	3
5	*ma*	hemp	2
	mah	to scold	4
6	*yaa*	hoarse	3
	yah	to press down	4

[13] I was not able to discover any difference between this Chinese type of whisper and any other kind of whisper which I have heard, e. g. in English or in Norwegian, as far as voicelessness is concerned.—The nature of the Mandarin tones is as follows: Tone 1, high level; Tone 2, high rise; Tone 3, low fall-rise; Tone 4, high-to-low fall.

[14] I am indebted to Mrs. R. C. Pian, Harvard University, for supplying the words to fill the requirements stated above.

Mr. K. C. C. was whispering, and Mrs. K. C. C. was listening. Recognition score: 88 %.

The 12 % of "incorrect" answers were the following fourteen out of 120:

far	2	was	heard	as	*faa*	3	six times
tah	4	—	—	—	*ta*	1	three times
lha	1	—	—	—	*la*	2	twice
mah	4	—	—	—	*ma*	2	once
ta	1	—	—	—	*tah*	4	once
ma	2	—	—	—	*mah*	4	once.

d. Same procedure and test material as in experiment 4c, but this time Mrs. K. C. C. whispered and Mr. K. C. C. listened. Recognition score: 73 %.

The 27 % of "incorrect" answers were the following thirty-two out of 120:

tah	4	was	heard	as	*ta*	1	nine times
far	2	—	—	—	*faa*	3	seven times
lha	1	—	—	—	*la*	2	six times
ba	1	—	—	—	*baa*	3	five times
faa	3	—	—	—	*far*	2	twice
ma	2	—	—	—	*mah*	4	once
yaa	3	—	—	—	*yah*	4	once
ba	1	—	—	—	*baa*	3	once.

Both subjects thought they might have made a few mistakes in whispering, "Mrs. C. perhaps more than Mr. C." No tape recorder was used in experiments 4c and d. The room was quiet. Being a married couple, the subjects were well acquainted with each other's speech.

Summary of Recognition Scores for Chinese (Mandarin)

Subject whispering	Subject listening aud recognizing	Experiment	Score
Mr. K. C. C.	Mr. K. C. C.	a	83 %
Mrs. K. C. C.	Mrs. K. C. C.	b	79 %
Mr. K. C. C.	Mrs. K. C. C.	c	88 %
Mrs. K. C. C.	Mr. K. C. C.	d	73 %

It will be seen that the Chinese subjects obtained far better scores than the Norwegian and Slovenian ones. Besides the reasons already mentioned, attention should be drawn to the non-pitch concomitants of Mandarin tones. As described by

M. Swadesh,[15] the tones also manifest differences in stress and length, which may act as cues for their recognition in whispered speech.

The conclusion seems unavoidable that word "tones" are reflected somehow in whispered speech, and better in some languages (Swedish, Mandarin) than in others (Norwegian, Slovenian). *How* they are reflected is an exceedingly difficult question, since we do not seem as yet to have the appropriate means for psycho-physical investigation of whisper. This is not to say that the sound spectrograph should not be used for such purposes: it may reveal something of interest, but only after careful evaluation of a great number of recordings.

It should not be forgotten that word "tone" is not a purely chromatic phenomenon; the frequency modulation is probably always coupled with changes in stress (either physiological or physical intensity, or loudness, or any combination of these). Whether or not in some cases the degree of stress is distinctive and the pitch merely concomitant to stress, is a matter of interpretation. To speak of stress features as substitutes for pitch is misleading, as these features also exist in voiced speech, and the spectrograms of whispered speech which we have seen so far do not seem to indicate anything about the manner in which stress features take the role of pitch in whisper. It is true that a vowel tends to have added noise when whispered in an intended rising tone, but some languages—among them Norwegian—combine stress with a low tone and have weaker stress on higher tones. Still, one would guess that a Norwegian whispering Meyer-Eppler's "sung" vowels would add noise to higher tones. As for the change in the location of formants in [a, u], it should be remembered that even in normal song the character of the vowels tends to change with the pitch. On the other hand, if we consider whistling, we know that the tone is raised or lowered by changing the configuration and volume of the mouth cavity. This must have been what Meyer-Eppler had in mind when he told his subjects to maintain "the quality of a given vowel as well as possible" (see the study referred to in footnote 9).

We still know very little about the relationship between physiological (muscular) intensity and pitch produced, or between

[15] Morris Swadesh, "A Condensed Account of Mandarin Phonetics," *Travaux du Cercle linguistique de Prague*, VIII (1939), 213-216, p. 214.

physical intensity (amplitude) and pitch perceived. As we progress in the study of the combination of stress and pitch characteristics, we must be prepared for the discovery of rules which are not common to all languages.

University of Bergen.

GRAMMATICAL COMPLEXITY AND APHASIC SPEECH

H. Goodglass and J. Hunt

Linguistics challenges the student of aphasia to determine whether various levels of language are disturbed in brain-injured persons differentially according to their inherent "complexity" in the language. In the domain of phonology R. Jakobson (1942) has suggested in quite specific terms that in aphasia, phonemes are dissolved in an order of greater to lesser complexity. We would not expect a French aphasic, for example, to maintain the complex distinction between /i/ and /ü/ or /e/ and /ẽ/ if he had lost the more fundamental distinction of /a/ and /i/. This theory of Jakobson's has unfortunately not been followed by the required experimental investigation.

The present paper[1] deals with the related problem of differentially disturbed levels, not in phonology, but in grammar. The problem of grammatical disturbance in aphasia is significant not only for linguistics, but for neurophysiology and clinical diagnosis as well. Differential impairment of the grammatical structure of spoken language has been regularly described in cases of aphasia and most authorities have distinguished between an "agrammatic" form, marked by simplification and loss of grammatical detail, and a "paragrammatic" form, marked by confused and incomplete, but not necessarily simplified constructions. The similarities and differences between these two speech patterns have not been adequately accounted for theoretically, nor has an objective empirical means of distinguishing between them been developed.

For the purpose of the present experiment, we have chosen two English inflectional morphemes: the noun plural and the possessive. Apart from minor allomorphs of the plural these

[1] The authors are indebted to Professor Morris Halle of the Massachusetts Institute of Technology for suggesting the general design of the experiment described here. The assistance of Uriel Weinreich in formulating the linguistic issues is gratefully acknowledged.

two morphemes have the same allomorphs, /-s, -z, and -əz/, in the same distribution. Thus, any differential treatment of the morphemes on the part of an aphasic cannot be due to a difference in phonemic form, but must be due to their different status in the grammatical system.

Our theory is that the possessive is more complex than the plural, and leads to the following specific hypotheses:

1. When they are given stimuli intended to elicit responses with plural nouns and responses with nouns in the possessive form, some aphasics should have more difficulty with the possessive form than with the plural, but none should have significantly more difficulty with the plural than with the possessive.

2. When required to discriminate incorrect usage of plural and of possessive word forms, some aphasics should make more errors with possessives, but none should make significantly more errors with plurals.

This prediction can be derived in several alternative ways and, for economy, the discussion of the theory has been combined with the discussion of the results in the final section of this paper.

The following secondary problems were also considered:

1. The final *s* indicating the third personal singular in verbs was included in the test for auditory discrimination of incorrect forms. It was predicted that this form, like the possessive *-'s*, should be more vulnerable than the plural *-s* to disturbance in aphasia.

2. It is important to determine whether the disturbance of grammatical discrimination in aphasia affects expressive and receptive language similarly in most patients, or whether it may appear in one modality without the other.

3. It is significant to determine whether difficulties with grammatical forms are only a reflection of the severity of aphasia, or whether there is evidence for specificity of this type of defect.

Procedure

Two experimental tests were used in this study: an Expressive Final S test and a Receptive Final S test. (These terms relate, respectively, to the expressive and receptive use of language, i.e. to speaking and writing, on the one hand, and to understanding on the other.) In each item of the Expressive test, a simple declarative sentence of from five to seven words was read to the

subject twice. The reading was followed immediately by two questions, the first of which required a one-word answer with a pluralized noun and the second an answer with a possessive form ending in -'*s*. Both responses were part of the original sentence. To illustrate:

Examiner reads: My sister lost her gloves. *(Repeated.)*
Question 1. What did she lose?
Question 2. Whose gloves were they?
Examiner reads: The baby dropped his toys. *(Repeated.)*
Question 1. What did he drop?
Question 2. Whose toys were they?

For the grading of an answer as "correct," it was necessary for the final *s* to be clearly heard, although a distorted pronunciation of the stem of the word was tolerated, and help with the initial sound of the word was given to patients with severe word-finding difficulty. The test consisted of twelve of these sets.

In the Receptive Final S test, 30 sentences of six to ten words each were divided into six categories, as follows:

A. Six sentences in which the final *s* is omitted from a possessive. E.g., *The ship anchor was lost in the storm.*

B. Six sentences in which the final *s* is omitted from a plural noun. E. g., *There were three book on the table.*

C. Six sentences in which the final *s* is omitted from a verb. E.g., *The soldier write home every week.*

X. Four correct sentences similar in structure to sentences of Type A.

Y. Four correct sentences similar in structure to sentences of Type B.

Z. Four correct sentences similar in structure to sentences of Type C.

These sentences were arranged in a random sequence and recorded on tape with each sentence repeated once, and a 15 second interval between the repetition and the first reading of the next sentence. The subject was instructed simply to listen carefully and to indicate whether each sentence sounded like correct English or not.

Selection of Subjects

Twenty of the 24 subjects used in this study were patients residing in the Aphasia Unit of the Boston Veterans Administration

Hospital and four were patients at the Lemuel Shattuck Hospital.[2] They ranged in age from 25 to 70 years and included all types of aphasia, ranging in severity from mild to severe. Ten patients were subjects in both the expressive and the receptive experiments. Five subjects were retained only for the receptive experiment because they made no errors on the Expressive Final S test and had virtually normal speech patterns. It was felt that inclusion of their scores would distort the results of the study with spuriously high correlations. Nine patients who participated in the expressive experiment were no longer available for the receptive experiment. The characteristics of the subjects are summarized in Table 1.

TABLE 1. CHARACTERISTICS OF SUBJECTS USED IN THESE STUDIES

Subject	*Age*	*Type of Aphasia*	*Severity*
		Subjects Common to Both Experiments	
Sar.	46	Mixed, predom. Express.	Severe
Hig.	27	Mixed, predom. Express.	Severe
Ray.	38	Predominantely Expressive	Severe
Bar.	27	Expressive	Severe
Ton.	38	Expressive	Moderate
Hic.	70	Receptive	Mild
Mur.	62	Receptive	Mild-moderate
Chu.	26	Mixed, predom. Express.	Moderate
Cou.	38	Mixed, predom. Express.	Moderate
Cru.	32	Expressive	Moderate
		Subjects used in Receptive Experiment Only	
Com.	36	Mixed	Mild residual
Fie.	32	Expressive	Mild
Phi.	58	Expressive	Mild
Tay.	34	Expressive	Mild residual
Pei.	59	Expressive	Mild residual
		Subjects Used in Expressive Experiment Only	
McC.	41	Mixed	Moderate-Severe
Pen.	38	Expressive	Moderate
Rol.	54	Expressive	Moderate

[2] The authors are indebted to Miss Mary Hyde, Speech Therapist at the Lemuel Shattuck Hospital, for her assistance in testing patients.

Subject	*Age*	*Type of Aphasia*	*Severity*
Del.	48	Mixed	Severe
Sla.		Mixed	Moderate-Severe
Murr.	62	Mixed	Moderate
Ren.	40	Expressive	Severe
Fei.	51	Expressive	Moderate-mild
Rya.	55	Expressive	Moderate

Results

Expressive Final S. Examination of the first specific hypothesis requires comparison of the number of errors in the use of plural as compared to possessive final *s* in the Expressive Final S test. The patients were easily dichotomized into those with high total errors (more than 4) and low total errors (less than 4). Table 2, summarizing the results on this test, is therefore presented with sub-totals for high- and low-error subjects. The difference in number of plural errors (37) and possessive errors (66), as tested by the Wilcoxon Signed Ranks method, is significant, with p less than .025.

By inspection, Table 2 reveals that the high total error group was consistently higher in possessive errors, while the low error group was equally divided in incidence of the two types of errors. The number of cases involved, however, is too small to rule out the effects of pure chance.

Table 2. Number of Errors in Expressive Final S Experiment

	Type of Error		
Subjects	*Plural*	*Possessive*	*Total*
High Error Group			
Ton.	0	8	8
Ray.	3	5	8
Bar.	7	9	16
McR.	5	8	13
Chu.	1	5	6
Hig.	1	4	5
Pen.	2	6	8
Rol.	8	11	19
Subtotal	27	56	83

Subjects	*Plural*	*Possessive*	*Total*
Low Error Group			
Cou.	0	0	0
Hic.	1	2	3
Cru.	2	1	3
Mur.	1	0	1
Del.	1	2	3
Sla.	1	2	3
Murr.	0	1	1
Ren.	1	0	1
Fei.	2	1	3
Rya.	1	1	2
Subtotal	10	10	20
Total	37	66	103

RECEPTIVE FINAL S. Examination of the second specific hypothesis requires comparison of the number of errors of types A, B, C, and XYZ (rejection of correct sentences). The data are summarized in Table 3. Application of the Wilcoxon Signed Ranks test to the comparison of errors yields the following:

A—B Significant beyond .05 level

A—C Not significant

C—B Significant beyond .01 level.

A—XYZ/2 Just short of .05 level of significance.

B—XYZ/2 Not significant

C—XYZ/2 Significant beyond .01 level.

No useful dichotomy of high- and low-error subjects could be made for the Receptive Final S Test. In these data, the error score XYZ/2 (rejection of correct sentences) represents a measure of the subjects' tendency to make random errors due to confusability or to a fluctuating level of comprehension. It is apparent that Type B (plural) errors are no more frequent than the random error level, and that errors in detecting the omitted final *s* for possessive and verb forms occur significantly more often. Thus, the two specific hypotheses concerning the order of difficulty of grammatical forms for aphasics are supported.

TABLE 3. NUMBER OF ERRORS IN RECEPTIVE FINAL S EXPERIMENT

	Type of Error			
Subjects	Type A *(Possessive)*	Type B *(Plural)*	Type C *(3rd Pers. Sing.)*	ΣXYZ
McG.	2	4	4	2
Sar.	0	2	2	4
Hig.	2	0	1	.5
Ray.	0	0	1	1.5
Bar.	3	1	3	2
Com.	0	0	1	0
Tay.	1	0	1	0
Pei.	1	0	1	.5
Ton.	3	0	2	1.5
Fie,	1	0	3	0
Phi.	0	0	1	0
Hic.	3	0	1	0
Mur.	2	0	1	0
Chu.	1	1	4	.5
Cou.	5	1	1	.5
Cru.	2	1	3	1.5
TOTAL	26	10	30	14.5

ΣXYZ represents the number of rejections among the 12 grammatically correct sentences. This figure is divided by two to make it comparable to the error scores for types A, B, and C which are based on sets of six sentences.

INTERCORRELATIONS AMONG THE TESTS. Computation of rank-order intercorrelations among the several subscores of the tests permits examination of the secondary problems raised in connection with this study. These correlations are summarized in Table 4. In this table, the Functional Speech score, taken from the clinical aphasia examination, is used as a measure of the general severity of the aphasic speech handicap.

It is apparent, from the low correlations in the last section of the table, that difficulties with the *expressive* use of the *-s* are unrelated, in our subjects, to difficulties in *perceiving auditorily* the omission of the corresponding *-s* in the context of a sentence. Severity of aphasia is significantly predictive of difficulty with the possessive *-'s* on the expressive side, but only doubtfully related

to difficulty with the plural *-s* (correlations of .53 and .38, respectively). On the receptive side, there is a sharp division in the clustering of the subscores. The XYZ, or random receptive error rank, is highly predictable from knowledge of the severity of aphasia (rho=.82). Errors in the perception of the plural and third person singular *-s* are also strongly related to the severity and to each other. On the other hand, the non-perception of errors with possessive *-'s* is insignificantly correlated either with the severity of aphasia or with the other receptive subscores.

TABLE 4. INTERCORRELATIONS AMONG THE SUBSCORES OF THE FINAL S TESTS

Correlations between:	N	rho	Significance
Expressive Final S			
Functional speech vs. possessive errors	14	.53	.05
Functional speech vs. plural errors	14	.38	n.s.
Possessive vs. plural errors	17	.48	.05
Receptive Final S			
Functional speech vs. Type A (poss.) err.	16	.32	n.s.
Functional speech vs. Type B (pl.) errors	16	.73	.01
Functional speech vs. Type C (3rd pers) err.	16	.68	.01
Functional speech vs. XYZ (random) errors	16	.82	.01
Type A vs. Type B	16	.32	n.s.
— A — C	16	.24	n.s.
— B — C	16	.75	.01
— A — XYZ	16	.25	n.s.
— B — XYZ	16	.75	.01
— C — YXZ	16	.60	.01
Expressive and Receptive			
Possessive vs Type A.	10	.08	n.s.
Plural vs. Type B	10	.32	n.s.
Possessive vs. XYZ	10	.78	.01
Plural vs. XYZ	10	.57	.05

Discussion

Jakobson's (1956) reformulation of agrammatism as "contiguity disorder" does not attempt a detailed prediction of the order of dissolution of grammatical processes in aphasia. However, his position does lead to the hypothesis that contiguity disorder first interferes with those obligatory grammatical forms which express syntactic relations and are not a matter of semantically determined choice. In the present experiment, the possessive and the third person verb suffixes are of this type, being highly determined by syntax, whereas the plural ending has a predominantly semantic function and is to a large degree independent of syntactic relationships. Thus, while this study is not a strong test of Jakobson's position, the results are in accord with his general formulation. In order to provide stronger support for an interpretation of the data in these terms, it would be necessary to produce parallel findings with other pairs of endings which may have either a semantic or an automatized, syntactic function in different settings. In English, these include the noun ending /ɪŋ/ (as in *clipping*, *beginning*) vs. the /ɪŋ/ of the verb in the progressive present; the /ər/ of agency *(seller, buyer)* vs. the comparative /ər/. The results should also discriminate patients who have the clinical speech pattern of agrammatism from the non-agrammatic.

Chomsky's theory of syntax (1957) provides specific predictions about the order of complexity of grammatical structures. According to this theory, some sentences in a language belong to a basic level ("kernel sentences"), whereas others are formed from kernel sentences by transformations. Elaborations or substitutions on the phrase structure level, which remain on the basic kernel level, are regarded as grammatical processes of a lower order than transformations.

Now, sentences containing possessives in English are not kernel sentences, but transforms of kernels. For example, *John's hat...* is a transform of the kernel sentence *John has a hat* and, in general, *X's Y*, wherever it occurs, is a transform of *X has Y*. The formation of the plural from a singular noun, on the other hand, is a process within the phrase structure level and is thus less complex. The results in the present study are consistent with Chomsky's position insofar as the noun plural was less disturbed than the grammatically more complex possessive. Perhaps a

stricter test of Chomsky's theory would compare the use of a transform with the particular kernel from which it was derived. The experimental advantage of making the responses phonetically identical could be retained, as in the following type of completion item:

The soldier mails a letter home every night. He did the same last night. What did he do with the letter? He———it. (Kernel sentence)

And what happened to the letter? It was——— (Passive transformation)

This line of investigation is now being followed intensively.

Chomsky's theory does not account for the patients' difficulties with the *-s* of the third person singular verb form. The addition of this suffix is a change at the phrase structure level, yet it was found more subject to disturbance than the possessive transformation. At the risk of introducing an additional principle for the explanation of a further fact, we may conjecture that another factor at work in determining the survival value of a grammatical structure is its redundancy. The *-s* of the verb form is completely determined by the subject of the verb, and its omission does not appreciably increase the ambiguity of the sentence in which it appears.

Our limited sample gives no support to the hypothesis that a common factor explains disturbances both in grammatical expression and in auditory discrimination of grammatical forms. It is true that the receptive task used here was not directly analogous to the expressive: it required only a recognition of the correct or incorrect-sounding sentence, without demonstration of response to the meaning. However, our finding is in line with Pick's observation (1931) that expressive and receptive agrammatism occurred independently of each other. It is interesting to note that the disturbance in *recognizing* the missing *-'s* is particularly poorly related to the other *-s* difficulties. However, in view of our small sample, it would be premature to offer explanations before the finding is verified in further research.

The present study demonstrates the need for more detailed investigations of agrammatism based on sound linguistic theory regarding the hierarchical structure of grammar. This approach

has proved fruitful in the present experiment and promises to add both to the understanding of aphasic symptoms and to the refinement of linguistic theory.

Boston Veterans Administration Hospital.

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ON THE PHONEMICS OF THE FRENCH CREOLE OF TRINIDAD

MORRIS F. GOODMAN

The segmental phonemes of Trinidad Creole may for convenience be arranged in the following manner:

p	t	č	k					i			u	
b	d	ǰ	g		y		w	e		o		
f	s	š		l					ɛ	ɔ		ɛ̃ ɔ̃
v	z	ž			h				a			ã
m	n		ŋ									

The oppositions between the consonants would not permit us to group any two or more of them within the same phoneme; some might possibly be analyzed as sequences of phonemes, but this problem will be dealt with when the phoneme sequence patterns of the language are treated as a whole. The vowels too must all be regarded as phonemically distinct, and none could reasonably be considered a sequence.

In the set of oral vowels, /e/ and /o/ occur only in open syllables, while all others occur also in closed ones. Consequently, the respective oppositions /e/ vs. /ɛ/ and /o/ vs. /ɔ/ might be regarded not as distinctions of tongue height (similar in principle to the difference between /e/ and /i/ or /o/ and /u/), but, rather, as separate though parallel binary contrasts involving a feature which is distinctive only in these two.[1] Even though /e/ and /o/ do not contrast with /ɛ/ and /ɔ/, respectively, in closed syllables, the vowels

[1] I could not say, however, what such a feature might be, and setting it up, I feel, would be somewhat arbitrary. Robert A. Hall Jr., faced with a similar problem in his *Haitian Creole*, sets up for these two pairs a distinction of tense vs. lax for the higher and lower members of the opposition respectively. I am uncertain as yet whether this distinction is applicable to the Creole of Trinidad on the basis of phonetic fact, but is should be pointed out that Hall's solution may have more structural motivation in Haitian than in Trinidadian Creole, since in the former there are five nasal vowels including (/ĩ/ and /ũ/) rather than three, so that by reducing the oral vowels to five positions (with /e/ and /ɛ/ as well as their back counterparts distinguished by manner, not place of articulation), the oral and nasal vowel patterns are made precisely congruent with one another.

occurring there should be grouped phonemically with the /ɛ/ and /ɔ/ of open syllables on the basis of far greater phonetic similarity. On the same basis, as well on that of greater similarity of distribution, the two mid nasal vowels [ɛ̃] and [ɔ̃] should be paired with /ɛ/ and /ɔ/; that is to say, they should be phonemicized as /ɛ̃/ and /ɔ̃/, not as /ẽ/ and /õ/.

Having ruled out the possibility of merging into a single phoneme any of the above listed consonants on the one hand, or vowels on the other, I implicitly left open the possibility of combining vowels with their corresponding semi-vowels. The following items would have a decisive bearing on any such decision: /kwi/ 'calabash', /kuy/ 'his neck', and /ku(w)i/ 'run'. It is true that /y/ follows /u/ in word final position only if it constitutes a separate morpheme indicating possession by the third person singular; but even if morphological considerations are to be admitted in principle, I would not consider this particular fact relevant, since sequences of other vowels and /y/ are not phonemically distinguished by the presence or absence of an intervening morpheme boundary (/kay/ 'house' is homophonous with /kay/ 'his case'.) The opposition of [kwi] and [kuy] makes it necessary to distinguish one vowel from its corresponding semi-vowel, but not both, nor does that pair indicate which semi-vowel should be treated as phonemic. The forms could be analyzed either as /kui/ and /kwi/, respectively, or as /kui/ and /kuy/, respectively. The third form, [ku(w)i], could be regarded as a sequence of two full vowels with or without an intervening semi-vowel (hence the *w* in parentheses). The latter solution would force us to distinguish both /y/ and /w/ from their syllabic counterparts, but it is the former solution that I consider the more reasonable, since every sequence in the language that could be regarded as one consisting of two syllabic vowels *without* an intervening semi-vowel could also be regarded as one *with* an intervening semi-vowel, while the converse is not true. Thus, pairs like /žu(w)e/ 'to swear' vs. /muye/ 'wet' and /mi(y)ɛt/ 'grain' vs. /mi(w)ɛ/ 'mirror' certainly force us to consider at least one member of each pair as a VSV sequence. It would be more reasonable, therefore, to consider all of them to be of this type, thereby achieving a more restricted and symmetrical set of permitted phoneme sequences (i.e. one that includes fewer types of sequences but utilizes more fully individual instances conforming to these types.) I thus prefer the analysis /peyi/ to /pei/ 'country', /hayi/ to /hai/ 'hate' etc.

There still remains the question, however, whether both or only one (and in that case, which one) of the semi-vowels must be

distinguished phonemically from their syllabic counterparts. A possible aid in solving the issue is the contrast between [ywit] 'eight' (the unique occurrence of this initial cluster which is reduced to /w/ in the speech of many) and [iwi] 'he laughed'. (The morphological fact that the latter from is composed of two words again has, in my opinion, no bearing on its phonemic relevance since in [diwi] 'rice' the same sequence occurs within a single morpheme). The opposition [ywit]:[iwi] demands a distinction between /y/ and /i/. Is one similarly demanded between /w/ and /u/? A form [twuve] 'find', though not recorded by me, appears in a phonemic transcription of a Trinidadian Creole text, *Pierrot Grenade* by Andrew Carr.[2] Its initial syllable would have to be distinguished from [tuw] 'your cough', thus forcing one also to distinguish /w/ from /u/, just as /y/ was distinguished from /i/.

The proposed solution is completely determinate only for prevocalic position: [kwa] 'cross' is definitely /kwa/ = CSV, and [pyɛ] 'foot' is /pye/, but is [kaw] 'your case' /kau/ = CVV or /kaw/ = CVS, and is [pɛy] 'fear him' /pɛy/ or /pɛi/? The phonetic nature of [w] and [y], and considerations of distributional symmetry, would indicate a semi-vowel solution. One reason to consider an alternative analysis would be that /y/ and /w/, as indicated above, in syllable final position often occur as morpheme alternants of /i/ and /u/, the third and second person singular pronouns respectively, and consequently such a solution might simplify the description of these morphophonemic alternations. But at the same time it would complicate the description of other morphophonemic alternations. For example, the postposed third person singular pronoun has two alternants, /li/ after consonants and /y/ after vowels (phrase initially it is a full vowel, /i/). The word /kay/ 'house' (homophonous with /kay/ 'his case') takes the post-consonantal form of the pronoun, i.e. *kayli* 'his house'; so does /kyaw/, 'a kind of marble', which rhymes with /kaw/ 'your case': *kyawli* 'his marble'. This is the only instance I know of where /w/ occurs in a word-final position as an integral part of the morpheme rather than as the second person singular pronoun. The semi-vowels, therefore, should be phonemicized as such both in post and prevocalic positions even where they are pronominal morphemes.

[2] *Caribbean Quarterly*, IV (1956), nos. 3 and 4, pp. 281-314. This example, I later discovered, turns out to be unconvincing, since the dialect represented there, unlike the one I describe, has a phonemic distinction between /r/ and /w/. The /r/, however, is also transcribed /w/, as in this example.

In this connection I should mention the morphophonemic alternation of the postposed definite article, which parallels closely that of the postposed third person singular pronoun. After consonants, including the semi-vowels, it is /la/, but after vowels it is a scarcely stressed, I might almost say non-syllabic, [a]. I therefore would prefer to regard it as a semi-vowel paralleling post-vocalic /y/ and /w/. Can it be tested in the same way as these two, i.e. in terms of the morphophonemic alternants which may follow it? Since it normally appears only as a separate postposed morpheme, apparently it cannot, but one (doubtless marginal) simplex word, /obɛa/ 'magic', which rhymes with the bi-morphemic /flɛa/ 'the flower', is followed by the *postconsonantal* alternants of the definite article and third person singular pronoun, /la/ and /li/ respectively. It seems reasonable, therefore, to consider this unstressed vowel [a] a semivowel, and to include it in one phoneme not with the full vowel /a/, but with the consonant /h/, which otherwise occurs only prevocalically. If pressed for common distinctive features between the post- and pre-vocalic allophones of the resultant phoneme, one might suggest minimal buccal constriction. /h/ thus becomes the semi-vocalic counterpart of /a/, precisely as /y/ and /w/ are those of /i/ and /u/ respectively. It can then be said of all three semi-vowels that their occurrence after their corresponding vowels is phonetically equivalent to a lengthening of the latter: (/fiy/ 'his daughter'—[fi:]; /tuw/ 'your cough'—[tu:]; /wah/ 'the king'—[wa:]). Consequently, in the case of the postposed definite article and third person singular pronoun, we have the following distribution of alternants: after consonants, -lV (where V = /a/ and /i/, respectively); after vowels, the corresponding S (i.e. /h/ and /y/ respectively).

I should say a few words about the types of permitted phoneme sequences in Creole, having alluded to them previously. Initially preceding a vowel there may occur zero, one, or two consonants. If two, the second must be /y/, /w/, or /l/; in this respect /h/ stands apart from the other two semi-vowels, occurring in no clusters either initially or finally. /l/ may precede /y/ or /w/, but may not follow them, and in the speech of some /y/ may precede /w/ in the one word, /ywit/ 'eight', but /wy/ does not occur. All labial consonants may precede all three aforementioned sonants, except that /m/ occurs only before /y/ and /w/; the dentals may precede /w/ and less commonly /y/ but not /l/ in the texts I have recorded, except in two forms: /sla/, a pre-pronominal possessive formative, and /dlo/ 'water' (though many say /glo/). The palatals may be followed by /w/ only, but /ž/ precedes /l/ in one word, /žle/

'unripe fallen fruit'. The two velar stops precede /w/ and /l/, and /y/ only in the case of the aforementioned /kyaw/.

Since in final position no consonant clusters occur, but all consonants do save /č/ and /ǰ/, one might be tempted to regard these two as clusters; on the analogy of other permitted clusters, these would most reasonably be /ky/ and /gy/ respectively. Since these affricates, [č] and [ǰ], do not occur before /a/ (and rarely before back vowels), the word /kyaw/ offers no impediment to such an analysis. However, the affricates do occur before the semivowel /w/ (e.g. /čwi/ 'leather'); were they analyzed as clusters, it would become necessary to posit initial clusters of three consonants, which otherwise do not occur, Hence, I find it more economical to regard these two as unit phonemes.

An investigation of the permitted phoneme sequences leads directly to the probleme of juncture, the phonemic indication of morpheme boundaries. In a sequence (C)VCV..., a morpheme boundary after the first vowel would not be phonemically indicated; a morpheme boundary after the second consonant may, however, be rendered phonetically. Compare, for instance, /tã-nu/ 'our time' and /tãn-u/ 'hear you'. I am not certain, however, if this distinction is always made in normal speech. In a sequence (C)VCCV..., if the two intervocalic consonants MAY NOT occur as an initial cluster, a morphemic boundary between them is not distinguished phonemically from its absence. If, on the other hand, the two consonants MAY occur as an initial cluster, then a morpheme boundary between them would be indicated phonemically. For example, /patwa/ 'patois', containing no morpheme boundary, is phonemically identical with /pa-twa/ 'by threes', in which a morpheme boundary immediately precedes the initially permitted cluster; /pat-wa/ 'paw of a king', however, with the boundary between the consonants, is phonetically distinct from the other two examples. (It will be recalled that *tw-* is permitted initially.) Thus a phoneme of juncture must be set up.

In closing I should say a word about distinctive stress. It clearly plays a role in this Creole. Compare, for example, /kɔ̃tˡli/ 'about him' and /ˡkɔ̃tli/ 'his story', or /deˡfwa/ 'sometimes' and /ˡdefwa/ 'twice'. Here the stress patterns involve units which extend beyond the boundaries of individual words. This state of affairs may possibly demand a somewhat complicated description, but I have not investigated it fully as yet and hence cannot propose a complete solution.

Columbia University.

THE INTERROGATIVE PARTICLE *-U* IN GERMANIC

OSCAR F. JONES

In discussions of Germanic morphology reference is made to an enclitic *-u*. Comments are ordinarily somewhat speculative, e.g., "... probably it [the Gothic 1st sing. opt.] is merely the indicative with the enclitic, either emphatic (adhortative) or interrogative particle *-u*..."[1] In most cases only superficial attempts are made to show that the proposed solution is in accord with a plausible concept of the particle's history. Since matters of considerable significance are involved—sound laws governing unaccented syllables, derivations of important endings, phonological theories—a more comprehensive treatment of enclitic *-u* would seem to be indicated.

The present paper is concerned with three questions: (1) Do any Gothic inflectional endings go back to a combination of final vowel and enclitic *-u?* (2) Are any North or West Germanic formations to be explained in this way? (3) Is there any reason to believe that *-u* functioned as a deictic particle in Germanic? We begin with a brief summary of previous viewpoints.

The idea of using enclitic *-u* to explain troublesome Gothic endings (the 1st sing. pres. opt., the entire passive opt., the 3rd person imperatives) seems to have been originated by Ernst Windisch.[2] It was picked up by Osthoff,[3] Brugmann,[4] Wiedemann,[5] and finally by the various handbooks.[6] It would not be exaggerating to call this interpretation the "standard" view.

[1] Eduard Prokosch, *A Comparative Germanic Grammar* (Philadelphia, 1939), p. 216.

[2] See H. Osthoff and K. Brugmann, *Morphologische Untersuchungen auf dem Gebiete der indogermanischen Sprachen* 4 (Leipzig, 1881), pp. 256, 405.

[3] *Ibid.*

[4] *Literarisches Centralblatt*, 1880, Sp. 943.

[5] *Litauisches Praeteritum* (Strassburg, 1891), p. 159.

[6] R. Bethge, in Dieter's *Laut- und Formenlehre der altgermanischen Dialekte* (Leipzig, 1900), pp. 372, 375; Friedrich von der Leyen, *Einführung in das Gotische* (Munich, 1908), pp. 39f.; A. G. van Hamel, *Gotisch Handboek* (Haarlem, 1923), p. 133;

Application of the hypothesis to North Germanic has found fewer advocates. Only one such explanation has been suggested, that the first singular of the Gothic optative *(-au)* and the Old Icelandic optative *(-a)* had a common source: the subjunctive ending *-ō* plus *-u*. This formulation was mentioned by Wiedemann in 1891 as an unacceptable alternative to an interpretation he was presenting.[7] Later it was recommended by Bethge,[8] Brugmann,[9] Boer,[10] and Kieckers.[11] Quite recently Wolfgang Krause[12] referred to the theory as a possibility and included the optative endings in a list of developments common to North and East Germanic.

Attempts have also been made to discover reflexes of enclitic *-u* in West Germanic. These have not been successful; the derivations proposed[13] have not been accepted or widely discussed.

Opinions regarding the function of the particle vary. We find it referred to as: "eine enklitische Partikel *-u* mit der Bedeutung der Unsicherheit und der Frage"; "*u* eine angehängte enklitische Partikel, vielleicht mit der Bedeutung 'wohl' und 'etwa'"; "*u*... a deictic particle"; "the emphasizing particle *-u*"; "die angehängte (hervorhebende) Partikel *-u*". Whether such descriptions are

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R. C. Boer, *Oergermaansch Handboek*[2] (Haarlem, 1924), pp. 275f.; Ernst Kieckers, *Handbuch der vergleichenden gotischen Grammatik* (Munich, 1928), pp. 192ff.; Prokosch, *Comp. Gmc. Gr.*, pp. 215f.; Wolfgang Krause, *Handbuch des Gotischen* (Munich, 1953), pp. 213f.; Joseph Wright, *Grammar of the Gothic Language*[2] (Oxford, 1954), p. 140.

[7] *Litauisches Praet.*, p. 159: "...aisl. *bera* geht... auf die sg. konj. idg. **bherām* zurück..., während got. *bairau* im sonderleben des gotischen aus **bairō* (= 1. sg. konj. idg. **bherām*)+u entstanden ist...."

[8] *Laut- u. Formenl. d. altg. D.*, p. 372.

[9] *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen*[2] 2.3 (Strassburg, 1916), p. 536 and *IF* 39 (1921), pp. 51ff.

[10] *Oerg. Hdb.*, p. 276.

[11] *Hdb. d. vgl. got. Gr.*, p. 192.

[12] *Hdb. d. Got.*, p. 48.

[13] Rudolf Meringer, *ZfvS* 28 (1887), p. 228, fn. 2: "Auch die anderen versuche [Meringer has been discussing Osthoff's derivation of a Sanskrit form, see *MU* 4, p. 258ff.] der glorification der partikel *u* sind Osthoff misglückt.... In ai. *ashṭāu* got. *ahtau* ein angehängtes *u* sehen zu wollen, das grenzt an das unglaubliche. Auch ahd. *zwiro* kann nicht *dvis u* sein; es setzt eine oxytonirte form voraus, wie ja O. selbst gesehen hat, und ist ferner kaum von *zwiron, en, ont, ent* und *zwiror*, an. *tysvar*... zu trennen." The suggestion made by Jellinek in *ZfdA* 39 (1895), pp. 128-9 that Gothic *aiþþau* 'or', 'else', OS *eftha eftho* OHG *eddo odo* derived from formations involving enclitic *-u* is not mentioned in the handbooks, not even in Jellinek's own *Geschichte der gotischen Sprache* (Berlin and Leipzig, 1926).

justified must be determined by an examination of the available source material.

First to be considered are occurrences of enclitic *-u* which can be regarded as completely uncontroversial.[14] There are thirty-two such passages in the extant Gothic texts, all quite obviously direct or indirect questions: (1) *skuldu ist kaisaragild giban kaisara* 'is it lawful to give tribute money to Caesar' (Mark 12.14); (2) *skuldu ist unsis kaisara gild giban* 'is it lawful for us to give tribute to Caesar' (Luke 20.22); (3) *skuldu ist in sabbatim þiuþ taujan* 'is it lawful to do good on sabbath days' (Mark 3.4); (4) *Fareisaieis frehun ina skuldu sijai mann qen afsatjan* 'the Pharisees asked him if it be lawful for a man to divorce [his] wife' (Mark 10.2); (5) *þau anþaranu wenjaima* 'or are we to expect another' (Luke 7.19); (6) the same (Luke 7.20); (7) *þau ainzu ik jah Barnabas ni habos waldufni* 'or do only I and Barnabas have no power' (1st Cor. 9.6); (8) and (9) *uzu waurstwam witodis ahman nemuþ þau uzu gahauseinai galaubeinais* 'did you receive the spirit from the works of the law or from the hearing of the faith' (Gal. 3.2) ; (10) and (11) *uzu waurstwam witodis þau uzu gahauseinai galaubeinais* 'from the works of the law or from the hearing of the faith' (Gal. 3.5); (12) *abu þus silbin þu þata qiþis* 'do you say this of yourself' (John 18.34); (13) *ei ufkunnau kustu izwarana sijaidu in allamma ufhausjandans* 'that I might know the proof of you whether you be obedient in everything' (2nd Cor. 2.9); (14) *silbans fragiþ sijaidu in galaubeinai* 'ask yourselves whether you be in the faith' (2nd Cor. 13.5); (15) *wileizu ei qiþaima fon atgaggai us himina* 'do you wish that we tell fire to come down from the heaven' (Luke 9.54); (16) *wileidu fraleitan izwis þana þiudan Iudaie* 'do you wish that the king of the Jews be released to you' (Mark 15.9); (17) *wileidu nu ei fraletau* 'do you wish then that I release' (John 18.39); (18) *wituda ha gatawida izwis* 'do you know what I have done to you' (John 13.12); (19) *maguslu driggkan* 'can you drink' (Mark 10.38); (20) *þau iku fram mis silbin rodja* 'or whether I speak of myself' (John 7.17); (21) *sau ist sa sunus izwar* 'is this your son' (John 9.19); (22) *hwas frawaurhta sau þau fadrein is* 'who sinned, this one or his parents' (John 9.2); (23) *ga-u-laubjats þatei magjau* 'do you believe that I can' (Matt. 9.28); (24) *þu ga-u-laubeis* 'do you believe' (John 9.35); (25) *frah ina ga-u-wa-*

[14] Not included in this figure are passages in which *-u* seems to be combined with enclitic *-h:* Matt. 11.3, John 7.17, Luke 20.4 (2 occurrences), Mark 11.30 (2 occurrences). For other materials of a controversial nature see below.

selvi 'he asked him whether he saw anything' (Mark 8.23); (26) *sunus mans qimands bi-u-gitai galaubein* 'coming shall the son of man find faith' (Luke 18.8); (27) *ei saihvam qimaiu Helias nasjan ina* 'that we see whether Elias comes to save him' (Matt. 27.49); (28) *ei saihvam qimaiu Helias athafjan ina* 'that we see whether Elias comes to take him down' (Mark 15.36); (29) *þankeiþ siaiu mahteigs* 'considers whether he be able' (Luke 14.31); (30) *rahneiþ manwiþo habaiu* 'reckons whether he have the necessary means' (Luke 14.28); (31) *witaidedun imma hailidediu sabbato daga* 'they watched him whether he would heal on the sabbath day' (Mark 3.2); (32) *swau andhafjis* 'do you answer thus' (John 18.22). The last twelve examples, with *-u* in postvocalic position, are particularly important for our purposes.

Parallel with these are sixty-six occurrences of *niu* '*nonne*'. All *niu*-passages combine negation with direct or indirect questions and are syntactically similar to material cited above. It can be assumed that *niu* is a compound made up of the negative adverb *ni* and the particle *-u*.

Jau 'whether, if' (nine occurrences) is usually explained as a compound of the same type. This may be correct,[15] but the alleged contraction of the particle with *u*[16] in *þu* 'you' (five occurrences claimed), *juþan* 'already' (one occurrence claimed) and *nu* 'now' (one occurrence claimed) is extremely dubious. Eliminating these questionable cases we have ninety-eight occurrences of the interrogative particle, seventy-eight of which have *-u* in postvocalic position.

Scholars who have cared to express an opinion regarding such passages are agreed that the particle constitutes a separate syllable in postvocalic as well as in postconsonantal position.[17] This conclusion would seem to be almost unescapable in view of the orthographic combination *aiu* in *qimaiu*, *siaiu*, *habaiu* (*nos.* 27-30 in

[15] Hermann Paul in *PBB* 4 (1877), pp. 385-6 gave a number of reasons for rejecting this explanation and suggested an interesting alternative.

[16] See Wilhelm Schulze in *ZfvS* 40 (1907), pp. 563ff.; M. H. Jellinek, *Ges. d. got. Spr.*, p. 96; Fernand Mossé, *Manuel de la langue gotique*[2] (Paris, 1956), p. 81.

[17] Adolf Holtzmann, *Altdeutsche Grammatik* (Leipzig, 1870), p. 9: "Das fragende *u* bleibt als besonderes betontes Wort nach allen Vocalen." Wilhelm Streitberg, *Gotisches Elementarbuch*[5-6] (Heidelberg, 1920), p. 78: "Zweisilbiges *i-u* ist in den Fällen anzunehmen, wo die Fragepartikel *-u* angefügt ist, z. B. in *ni-u, bi-u-gitai...*"; p. 77: "Zweisilbiges *a-u...*, wo die Fragepartikel *-u* angefügt ist, z. B. in *sa-u, swa-u ga-u-laubeis* usw." See also Wilhelm Braune, *Gotische Grammatik*[15] (Tübingen, 1956), pp. 14, 20; and Mossé *Man. d. l. lang. got.*, p. 45.

the list given above), which must be interpreted as a method of writing disyllabic sequences.[18]

It is difficult to see how a formation like the one we have been examining could combine with unaccented vowels to become a permanent part of the verbal suffix system. The following steps would necessarily be involved: (1) heterosyllabic *u* in certain interrogative locutions would have to become tautosyllabic; (2) consciousness of the semantic relationship between the contracted form and the heterosyllabic *u*, which was still in use, would have to be obliterated (recutting); (3) the contracted form would have to be leveled against all divergent endings.

There is serious doubt whether the first two steps in this sequence can be reconciled with a realistic analysis of Gothic structure. The material cited makes it necessary to assume that postvocalic allomorphs of the interrogative *u*-morpheme constituted independent syllables in the language of the scribes. It is probably safe to postulate a similar morphophonemic pattern for Wulfila's dialect and for earlier stages in the language's development. This observation is important. Unless we find evidence suggesting that the Germanic enclitic could appear as a non-syllabic we have no reason to assume that a single contraction took place. And a contraction of some sort would necessarily be the starting point for the whole development. As long as the interrogative particle was a syllabic in all environments there could be no confusion regarding its function; recutting would be impossible.

Support for the third step in the sequence is completely lacking. We cannot assume that the particle accompanied a major portion of the interrogative utterances, and even if we could, we would be far from creating a situation of the type usually associated with the leveling process. Optative forms used for questions would be outnumbered by optatives used for other purposes. The imperative would not fit into the scheme at all. Since this mood

[18] In a number of interesting passages a verb or adjective beginning with a vowel follows a formation which would ordinarily end in unaccented *a*, e. g., *ni kar ist* 'there is no care' (John 10.13) for *ni kara ist*, *þat ist* (Rom. 9.9) for *þala ist* 'this is', *ni þat-ain* 'not only this (that)' (Rom. 9.10, 2nd Cor. 8.19) for *ni þala ain*. We encounter the same apocope before enclitic *-ei* in the relative pronouns, e. g., *ana þammei* 'on which' (Mark 11.2) instead of **þamma-ei*, *þanei* 'which' (Mark 10.38) instead of **þana-ei* and before enclitic *-u* in *anþaranu* (nos. 5 and 6 in the list) for **anþaranau*. This suggests that unaccented *a* followed by the interrogative particle did not yield [au] in the scribal dialect and probably did not do so even in Pre-Gothic.

is never used in Germanic for the main verb of a question, third-person imperatives cannot be derived directly from formulae which depend upon the interrogative particle.

The question of word order introduces further complications. In our Gothic texts the particle *-u* follows the first or (much less frequently) the second element of the clause. Assuming that the pattern was similar at the time contraction occurred would limit possibilities drastically. Unless the word order brought a finite verb with the proper ending into position directly before *-u*, results would be negative. Questions in which the enclitic came after a verb with a terminal consonant (see nos. 13-19) would be obstacles in the path of the leveling process. Questions in which the particle followed a participle (nos. 1-4), a verbal prefix (nos. 23-26), a pronoun (nos. 20-22), an adjective (nos. 5-7), a preposition (nos. 8-12), or an adverb (no. 32) would have the same effect. The compounded endings could occur in clauses of this type only as the result of analogy. To argue that the new forms found their way into such questions and that the altered paradigms were then extended to non-interrogative forms, would be stretching the long thread of analogy to the breaking point.

Boer[19] tries to make the contraction theory more acceptable by suggesting motivation for analogical extensions of a different type. The development is assumed to have begun with the optative passive where we find the most forms ending in *au* and where *-u* functioned as a means of expressing the uncertainty which is characteristic of the mood. Since the optative had imperative connotations, the compounded ending would be extended to the third-person imperatives, thereby producing a distinction between these and comparable forms of the passive voice active. The suffix *-au* would also be taken over by the first-person optative, which in its turn would influence the preterit optative. How this complicated series of analogical reactions could have had its inception in the optative passive is not clear. The comparative frequency of passive forms must have been very low even at the time the contraction is supposed to have taken place.

Yet rejecting such explanations forces us to take up another problem, the chronological factor. Unless it is assumed that the contracted ending originated in a single paradigm and was

[19] *Oerg. Hdb.*, p. 275-6.

extended by analogy to the others, a number of separate contractions must be stipulated, *-u* combining in each with a vowel supplied by the paradigm in question. The optative passive formations—*-aizau* (2nd sing.), *-aidau* (3rd sing.), *-aindau* (3rd pl.)—are ordinarily based on the reconstructions: *-oīso* (Gk. *phéoio*), *-oīlo* (Gk. *phéroilo*, Sk. *bhárēla*), *-oīnlo* (Gk. *phéroinlo*). The enclitic would have to contract with the short vowel of the ending before the latter was lost. At that time, however, we would expect reflexes of unaccented IE *ō* to be long. Since proponents of the contraction theory base the active formations on reconstructions which involve *ō*—the 1st sing. opt. *-au* on *-ō* (Gk. *phérō*), the 3rd sing. imperative *-(a)dau* on *-tōd* (early Latin *datōd*), the 3rd plur. imperative *-andau* on *-ontōd* (Gk. dial. *pherónlō*) —setting up a transition stage *-ōu* for this group of endings is almost unavoidable.[20] Subsequent leveling is necessary to explain how reflexes of *-au* and *-ōu* could be identical.[21]

It might be better to assume that the passive endings resulted from a relatively early contraction, the active endings from a relatively late one. But this would be hard to reconcile with the fact that *-u* was in constant use as an interrogative particle throughout the entire period. Under such conditions a single series of contractions would be surprising.[22] An exact duplication

[20] Bethge, *Laut- u. Forml. d. allg. D.*, p. 372: "Die verschleifung muss sehr alt sein. älter als der schwund auslautender kurzer ultimavokale..., aber jünger als der ug. schwund auslautender idg. dentale.... Danach wird also *bairau* an. *bera* aus ug. **ƀerō+u* herzuleiten ... sein...." If we assume that contraction with *-u* prevented loss of a short ultima in the optative passive, we should be prepared to explain why the interrogative particle did not contract with the vocalic ending of the 2nd plur. opt. pres. e. g., **bherolle* (Gk. *phéroĭle*) preventing apocope of this ending. In a morphological tour de force, Brugmann, *IF* 39 (1921), pp. 51-2, attempts to avoid all such difficulties: "Erst nachdem der Nasal im Auslaut verklungen war [he derives the 3rd-pers. imperatives, the 3rd-pers. passives, both optative and indicative, from IE formations with *-ām*], wurde im konjunktivisch-imperativischen Gebiet die Partikel *u* fest mit der Verbalform univerbiert. Dieser Verschmelzung parallel ging die Entstehung der 1. Sing. auf *-au*.... Nachdem urgerm. **-dō̯*, **-ndō̯* in den Indikativ gelangt waren, setzte sich im Gotischen *u* im konjunktivisch-imperativischen Bereich um so leichter dauernd fest, als dadurch formale Scheidung zwischen dem uns nur in *atsteigadau*, *lausjadau*, *liugandau* aufbewahrten Formsystem und den zugehörigen Indikativformen erzielt wurde." See also Wright, *Gr. Goth. Lang.*, p. 140.

[21] Kieckers, *Hdb. d. vgl. got. Gr.*, p. 194.

[22] Edward H. Sehrt in *Fragen und Forschungen im Bereich und Umkreis der germanischen Philologie, Festgabe für Theodor Frings* (Berlin, 1956), p 7: "...es ist doch sehr zweifelhaft, ob man hier eine fest verwachsene Partikel annehmen darf, die sonst noch selbständig auftritt...."

of the phenomenon after a considerable lapse of time would be an unprecedented coincidence.

From what has been said it will be seen that the case for contractions involving interrogative *-u* is weak. To eliminate the worst complications, some scholars have suggested that the Germanic enclitic could function as a deictic particle. Support for the argument is sought in non-Germanic testimony: the enclitic is said to be cognate with a particle *u*, *ū*, 'and', 'also' which occurs in Sanskrit after verbs and pronouns, in *ulá... ulá* 'both... and', perhaps in Greek *pánu* 'altogether', 'at all', *ēúle* 'as well as'[23] and in Old Church Slavic *bereltŭ* 'he bears', *berątŭ* 'they bear'.[24] This etymology is not entirely satisfactory because of (1) seemingly unreconcilable differences in meaning between interrogative *-u* as it occurs in Gothic and its alleged cognates, (2) the problem of fitting the Gothic particle *-uh* 'and', 'but', 'now', 'therefore' into the same formulation. Hermann Hirt managed to avoid both difficulties by presenting more complicated explanations. He suggested that *-uh* would be derived from *-u* (cognate with Sanskrit *u*) combined with $k^{w}e$ (cognate with Sk. *ca* 'and', Greek *te*, Latin *-que*); that the Gothic interrogative particle could be derived in part from **n̥* the zero grade of **ne* (as in Latin *-ne*) and in part from **we* (as in Latin *ve* 'or').[25]

However this may be, we can be quite certain that early Germanic had two semantically independent formations: (1) the morpheme *-u* functioning as an interrogative particle, (2) the morpheme *-uh* functioning as an emphatic particle and as a conjunction. The latter seems to have had several allomorphs, viz., /uh, h, uþ, þ/, but it did not have an allomorph /u/. Claiming that Germanic had an emphatic particle *-u* which could be added to imperative and optative forms to strengthen them[26] regardless of where these

[23] Osthoff, *MU* 4.253; but see also Brugmann, *Die Ausdrücke für den Begriff der Totalität in den indogermanischen Sprachen* (Leipzig, 1894), pp. 63-4.

[24] Brugmann, *ZfvS* 27 (1885), p. 419.

[25] *Handbuch des Urgermanischen* 3 (Heidelberg, 1934), p. 192. For other explanations of *-uh* see Lidén, *Ark. f. nord. fil.* 4 (1888), p. 99ff. For Hirt's earlier discussion of the problem see *PBB* 18 (1894), pp. 299f.

[26] Joseph Wright, *Gr. Goth. Lang.*, p. 140; Kieckers, *Hdb. d. vgl. got. Gr.*, p. 193; Prokosch, *Comp. Gmc. Gr.*, p. 216. Hirt opposed arguments of this sort: "Diese Partikel *u* [i. e. interrogative *-u*] ist aber ein lebendiger Faktor..., während jene zur Verbalerklärung postulierte Partikel *u* nirgends nachweisbar ist...." (*IF* 6 [1896], p. 63); "Jellinek bietet keine Erklärung der got. Formen, denn die Partikel *u*, die er Btr. 95 heranzieht, pflegt sich überall da einzustellen, wo eine Deutung nicht gefunden ist." (*IF* 6, p. 59).

forms occurred in the sentence is a circular argument. There is nothing to justify it except the belief that the verbal endings we have been discussing go back to contracted forms.

It would seem safe to assume that the Germanic particle *-u* was used solely for questions and that even here it was very much restricted. The occurrence of enclitic *-u* in questions introduced by interrogative pronouns and adjectives or by interrogative adverbs of time, manner, and place must have been rare.[27] That more specialized formations encroached on the *u*-particle in some of the remaining interrogative possibilities is suggested by the following: (1) *Waitei ik Iudaius im* 'am I a Jew then' for *mêli* 'whether perchance' *egò Ioudaîos eimí* (John 18.35). *Waitei* occurs in one other passage, a declarative sentence in which it translates *tukhón* 'perhaps' (1st Cor. 16.6). (2) *An nuh þiudans is þu* 'are you a king then' for *oukoûn basileùs eî sú* (John 18.37). Elsewhere *an* is combined with the interrogative pronouns *ƕa* 'what' (Luke 3.10) and *ƕas* 'who' (John 9.36, Luke 10.29, 18.26). (3) *Ibai (num)* used to introduce ironic questions, e.g., John 8.53, and questions which presuppose negative answers, e.g., Rom. 11.1 (positive answers when followed by *ni*, e.g., 1st Cor. 9.4). There are also passages where the sense is 'lest that'. (4) *Nibai* used to introduce bitterly ironic questions, e.g., John 7.35, 8.22. It also occurs in non-interrogative clauses where the meaning is 'unless', 'except', 'if... not'.

Our Gothic texts contain approximately four hundred sixty direct questions. Of these, sixty-five make use of *niu*, twenty-seven (including five of the passages mentioned in fn. 14) make use of *-u*, and thirty-one are unaccompanied by specific interrogative labels of any sort. As they stand in the manuscripts these questions can be distinguished from comparable declarative sentences only through the context, e.g.: *gasaihvis þo qinon* 'do you see this woman' (Luke 7.44); *þata izwis gamarzeiþ* 'does this offend you' (John 6.61); *swa filu gawunnuþ sware* 'have you suffered so much in vain' (Gal. 3.4); *aftra þugkeiþ izwis ei sunjoma uns wiþra izwis* 'again does it seem to you that we excuse ourselves to you' (2nd Cor. 12.19); *anastodjandans ahmin nu leika usliuhiþ*

[27] There is one example in my list: no. 22. Adalbert Bezzenberger (*Untersuchungen über die gotischen Adverbien und Partikeln* [Halle, 1873], p. 98) assumed that interrogative *-u* was followed by the particle *-h* in *ƕa-uþ-þan* 'and what' (1st Cor. 4.7) and in *ƕouþ-þan samaqisse* 'and what agreement' (2nd Cor. 6.15, 16) but this is merely an attempt to make these passages fit into an alternation formula: *ûh*∾*-h*, *-ûþ*∾*-þ*.

'beginning in the spirit do you now finish in the flesh' (Gal. 3.3); *fimf tiguns jere nauh ni habais jah Abraham sahv* 'you are not yet fifty years old and have you seen Abraham' (John 8.57). It is possible to find passages beginning with *niu* which are directly parallel with others which have *ni:* (1) *niu ussuggwuþ aiw* 'haven't you ever read' (Mark 2.25), *ni þata ussuggwud* 'haven't you read this' (Luke 6.3, see also Mark 12.10); (2) *niu andhafjis ni waiht* 'are you making no answer' (Mark 15.4), *du mis ni rodeis* 'aren't you speaking to me' (John 19.10); passages with and without *-u: þu ga-u-laubeis du sunau gudis* 'do you believe in the Son of God' (John 9.35), *galaubeis þata* 'do you believe this' (John 11.26, also see John 16.31).

The occurrence of questions of this type without *-u* must mean that the contrast between interrogative and declarative intonation patterns made the particle virtually redundant. It is possible that increased dependence on contrasting intonation patterns was one of the developments which brought about the loss of interrogative *-u* in early North and West Germanic. We have no way of knowing and we have no way of fixing the date of the loss. That the particle is not documented in the earliest monuments does not necessarily mean that it did not occur sporadically in colloquial speech at that time. The North Germanic Runic sources would be a poor medium for preserving such syntactic minutiae, the earliest West Germanic glosses a still poorer.

On the other hand, there is no reason to suppose that any North Germanic endings go back to forms involving the interrogative particle. Nothing has been cited in support of this contention except parallelism between Gothic and Old Icelandic optative endings. That the Gothic evidence fails to justify such assumptions should be evident from the material presented above.

Considering the weaknesses in documentation, it is surprising that so many scholars have found it possible to accept the contraction hypothesis in one form or another. Perhaps the reason lies in the neatness with which this explanation takes care of troublesome morphological problems without upsetting the basic traditions of Germanic phonology. Some of these traditions need to be challenged. In another article[28] I have attempted to show that the diphthong [au] did not occur in the

[28] *Language* 34 (1958), pp. 33-9.

inflectional syllables of fourth-century Gothic. The conclusions reached in the present paper support this interpretation. Unless recourse is had to a particle, it is extremely difficult to account for [au]-values in the Gothic verbal endings.[29]

It would seem best to concede that setting up the *u*-contraction with [au] as its Gothic reflex was a step in the wrong direction and to admit the necessity for new formulations. Such formulations will almost inevitably bring us into conflict with the traditional derivations of certain Gothic and Old Icelandic endings, but this is surely no reason for avoiding a line of inquiry which promises to be fruitful.

University of Florida.

[29] Herbert Penzl, *JEGP* 49 (1950), p. 228, fn.: "Only the assumption of a ubiquitous particle *u* could explain a diphthong in *bairau*, *bairadau*, etc...."

THE *a*/*æ* OPPOSITION IN OLD ENGLISH*

SEYMOUR CHATMAN

1. Recent Old English phonemicists[1] have argued at length about the *ea*/*æ* opposition, but little has been said about the opposition *æ*/*a*.[2] Everyone seems to assume that such an opposition existed, but some have expressed a difficulty in finding conclusive minimal pairs of graphs:

> Instances of <a> in minimal contrast with <æ> in every occurrence of a pair of items are nearly impossible to find. That is, <a> and <æ> may well overlap so much as to make them

* I am indebted to Sherman Kuhn, Henry Hoenigswald and Harold Stine for reading and commenting on earlier versions of this paper.

[1] M. Daunt, "Old English Sound Changes Reconsidered in Relation to Scribal Tradition and Practice," *Transactions of the Philological Society* (1939), pp. 108-137; R. Stockwell and C. Westbrook Barritt, *Some Old English Graphemic-Phonemic Correspondences*—ae, ea, *and* a, *Studies in Linguistics, Occasional Papers*, No. 4 (Washington: 1951); M. Daunt, "Some Notes on Old English Phonology," *Transactions of the Philological Society* (1952), pp. 48-54; M. L. Samuels, "The Study of Old Elglish Phonology", *ibid.* (1952), pp. 15-47; S. M. Kuhn and Randolph Quirk, "Some Recent Interpretations of Old English Digraph Spellings," *Language* XXIX (1953), 143-156; R. Stockwell and C. W. Barritt, "The Old English Short Digraphs: Some Considerations," *Language* XXXI (1955), 372-389; S. M. Kuhn and Randolph Quirk, "The Old English Digraphs: a Reply,' *Language* XXXI (1955), 390-401; R. P. Stockwell, "The Phonology of Old English: a Structural Sketch," *Studies in Linguistics* XIII (1958), 13-24; and John A. Nist, "Phonemics and Distinctive Features of *Beowulf*," *Studies in Linguistics* XIII (1958), 25-33. The following abbreviations are used here: a. = accusative Angl. = Anglian, C = non-nasal consonant, Cp. = *Corpus Glossary* (eighth century, Mercian), d. = dative, Ep. = *Epinal Glossary* (eighth century, Mercian), Erf. = *Erfurt Glossary* (ninth century, Mercian), g. = genitive, Kent. = Kentish, Leyd. Gl. = *Leyden Glossary* (ninth century, Merc.), Lind. = *Lindisfarne Gospels* (tenth century, Nh.), Merc. = Mercian, n. = nominative, N = nasal consonant, Nh. = Northumbrian, p. = plural, Ru[1] = *Rushworth Ms.* (tenth century, Merc.), s. = singular, V = vowel, VP = *Vespasian Psaller and Hymns* (ninth century, Merc.), WS = West Saxon.

[2] After this paper had been completed, my attention was called to an article by Alan S. C. Ross entitled "Old English *æ* ∾ *a*," *English Studies* XXXII (1951), 49-56, in which arguments to explain the opposition of these two phonemes are proposed. Prof. Ross' approach and conclusions and mine, however, are sufficiently divergent to

appear to be in free graphic variation and possibly allophones of a single phoneme. Yet... there are several distributional facts that seem to deny the possibility of including what was written with *a* among the allophones of /æ/....[3]

It is true that there are not a great number of minimal orthographic pairs, but there are convincing reasons, based not only on distributional evidence but on well-known sound-change evidence

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warrant the publication of the present article. The differences consist of the following important points: (1) Ross' study is designed wholly to explain the occurrences of "analogical" forms, chiefly the oppositions between *o*-stem and *ā*-stem nouns (e. g., *stæle* 'place', d. s., vs. agds. nap. *stale* 'theft') and between *o*-stem nouns and verbs *fær* 'trip' vs. *fare* 'I go', see pp. 49 and 53). My paper concerns these forms only incidentally (see Sec. 12 below), and, in fact, tries mainly to show how normal phonological developments were responsible for the development of the opposition in forms where analogy is not ordinarily assumed. (2) Ross' point is that the so-called "irregular" *a* before front vowels in these paradigms may not have been analogical at all, but rather the reflex of a normal phonological development. He derives this conclusion by hypothesizing a new sound change in the unstressed Gmc. vowel system, namely Pgmc. *-ai-* could become not only *-ǣ-* (whence *-æ-*>*-e-*) as usually assumed, but also *-ā-*. This *-ā-* is first shortened and then levelled to *-e-*, but presumably after the *æ* of the stressed syllable had been darkened to *a* (pp. 54-56). Thus *a... e* is a normal environment in these paradigms. This theory is very interesting and may indeed explain certain things which have been written off a little too easily as analogy. However, the present paper tries to explain the development of the *a*/*æ* opposition without recourse to new sound-changes but strictly in terms of already accepted doctrine. (3) Ross' argument is limited entirely to West Saxon; in fact, he suggests "that the West Saxon state of affairs with its phonematic difference *æ*/*a* is not reproduced in the other dialects" (p. 51). I am forced to disagree with this conclusion in view of Anglian forms like *fallan* 'to fall' vs. *fællan* 'to fell', *wæll* 'well' vs. *wall* 'wall', *wærma(n)* 'to warm' vs. *warma* 'warm' (wk. adj.), etc. (see further section 7 below). Ross' suggestion that the *a*/*æ* opposition did not exist in PrOE is at odds with the conclusions to be derived from the tables in section 3 below, as well as from my paper as a whole. In sum, it seems to me that there is ample evidence to support an early development of the *a*/*æ* opposition which was partially but not entirely obscured by later changes in non-WS dialects.

[3] Stockwell-Barritt, footnote 8, pp. 37-8 (1951). Stockwell and Barritt offer some circumstantial evidence which can be summarized briefly. (1) The presence of *æ* before CC plus back vowel where *e* was the usual product of the *i*-umlaut of Wgmc *a*: Proto-OE *æ*, e. g. *læccan* 'to seize', *gemæcca* 'mate', *wræcca* 'exile'. This point is called "difficult" by J. Wright, *Old English Grammar* (London: Oxford University Press, 1914[2]), § 55.3, who cites also the forms *sæcc* 'strife', *wæcce* 'vigil', *pæþþan* 'to traverse', *hæcc* 'gate', etc. K. Luick, *Historische Grammatik der englischen Sprache* (Leipzig: Tauchnitz, 1914), § 189.2aα calls *næss* 'ness' and *slæþþan* 'to support' "unklar". K. Bülbring, *Altenglisches Elementarbuch* (Heidelberg: Winter, 1902), § 177, explains the *æ* as a reflex of an original levelling of *a* for *æ* before the operation of *i*-Umlaut, citing Ru.[1] *sæcgan* 'to say' <**saggjan*, on the analogy of 2nd. and 3d. sg. pres. ind. and 2nd. sg. imp. forms like *sagas*, *sagaþ* and *saga*; similarly WS *hæbbe*,

as well, for acknowledging a phonemic opposition. Furthermore, it is a useful and necessary task in phonology to trace the genesis of such oppositions and to determine the conditions under which they were established.

2. There is good reason to assume an *a*/*æ* opposition if one considers the total picture of the PrOE vowel system:

i	*u*	*ī*	*ū*
e	*o*	*ē*	*ō*
( )	( )	*æ*	*ā*

The pressure toward symmetry of the phonemic opposition of front vs. back among the long vowels is clearly exerted towards the development of a similar opposition between short *a* and *æ*.

3. The handbooks, of course, do not directly concern themselves with oppositions. They are chiefly concerned with describing the conditions of changes in phones. Nevertheless, the necessary facts and conclusions concerning the origin of the *a*/*æ* opposition can be adduced from the handbook statements. The most authoritative formulation seems to be Luick's, which is as follows:

(a) Wgmc *a* became *æ* in all positions (except before nasals and in some weakly stressed syllables) (§ 116).[4]

(b) *æ* was preserved in WS, Nh., and part of Merc. (Ru[1]) in monosyllabic words ending in any single consonant (except *w* or *h*): *smæl* 'small', *æt* 'at', *bæc* 'back', *glæd* 'glad', etc., or in any consonant cluster (except *r*C, *l*C, or *h*C): *cræft* 'strength', *hælt*

WS *wæcce* 'watch', Angl. *hlæhhan* 'to laugh' and *slæppan* 'to step'. (2) "The preservation (by analogy with the infinitive, see Wright, § 58 note) of *a* in the present participle and gerund of class VI strong verbs..." *farende* for **færende* and *farenne* for **færenne*. [It is important to recognize these as normal phonological developments of *a* preserved before original *a*+N, not as analogies. See below, as well as E. Sievers-K. Brunner, *Altenglische Grammatik* (Halle/Saale: Niemeyer, 1942), § 363.2, § 363.3.] (3) "The regular alternation of *a* and *o* before nasals" which offers a parallel "methodological principle... that the reduction of opposition in a specific environment alone does not establish complementary distribution...." (4) "Finally... the differing development of <ae> and <a> after lengthening" (thus lengthened *a* gives MnE /ow/ or /ey/ while lengthened *æ* gives /iy/).

[4] Luick, § 116. Luick defends this assumption in § 164 against the older notion (e. g., Bülbring, § 127) that *a* is "preserved" in some positions originally. His argument is that if *a had* been preserved, then words like **slahan*, **parruk* and **gallo* would not have shown later breaking to **sleahan* (later *slēan*), *pearroc*, *gealla* but would have preserved *a* (as did *dragan*, *hassuc* and *crabba*). He feels that the breaking in these words to *ea* is strong proof of an original *æ* and that the suggestion that *a* could have become *ea* directly is invalid (see § 140). Later grammarians like Sievers-Brunner

'hat', *wæfs* 'wasp', *fæsl* 'fast'; and in polysyllabic words where the following syllabic was *e* from either original *æ* or from a syllabic consonant, or where the syllabic consonant itself was preserved: *fæder* 'father', *mægeð* 'maid', *æcer* 'acre', *fæðm* 'fathom', *mægden* (<**mægædin*) 'maiden' (§ 116), or in some forms where the following syllabic is *i* (**nærian* rather than **narian*, **æwi* rather than **awi*, etc., § 149).

(c) *æ* reverted to *a* before certain consonants: (1) before *w* except where followed by *i*: *clawe* 'claw's' (g.s.), *awel* 'awl', but *ewe* (from **æwi*) 'ewe' (§ 145, § 149); (2) before *ll* and *l*C in Anglian (and also some early WS documents and Kentish Urkunden), except where *ll* is the product of **lj* (§ 149): *all* 'all', *hall* 'hall', *ald* 'old' (§ 146), and in Merc. before single *l* not followed by i: *galend* 'singing', *hwales* 'whale's' (g.s.) (§ 146); (3) in Anglian (alongside *ea*) before *rr* and *r*C, particularly in Nh.: *arm* 'arm', *þarf* 'need', *warp* 'he threw' (alongside *earm*, *þearf*, *wearp*) (§ 147).

(d) Later, *æ* became *a* before back vowels, which remained in historical times in WS, Nh. and E. Merc. in the following positions: (1) in open syllables (except before N or *h*): *hara* 'hare', *talu* 'tale', *apa* 'ape', *sadol* 'saddle', etc. (§ 161.1); (2) before geminate consonants (except NN, *hh*, *rr*, *ll*): *hnappian* 'to nap', *malluc* 'fork', *racca* 'part of rigging of a ship', *crabba* 'crab', etc. (§ 161.2); (3) often before *s*C and *f*C: *wascan* 'to wash', *wrastlian* 'to wrestle', *saftriende* 'rheumatic', etc. (§ 161.3); (4) before obstruent consonants plus liquids, although very few forms are extant, since much levelling had taken place: WS *ap(p)la* 'apple', Angl. *ac(c)ras* 'acres' and LWS *caferas* (from **cafras*) 'beetle' (§ 161.4). The implications of this last group of forms to Luick are that "dieser Einfluss eines dunklen Folgevokals [ist] unabhängig von der Art der dazwischen stehenden Konsonanten."[5]

(§ 50.1 Anm. 4) and E. Kieckers, *Altenglische Grammatik* (Munchen: Max Hueber, 1935), § 5, support this view.

From the phonemic point of view, however, the issue is not crucial. That is, whether one follows the older view that $a < \frac{a}{æ}$ or Luick's that $a \rightarrow æ < \frac{a}{æ}$, the developments are only allophonic anyway until outside factors establish the *a*/*æ* contrast. If, as is suggested below, the establishment of the contrast occurred in the 7th century after *i*-Umlaut and changes in unstressed vowels, these 3rd-4th century changes have no phonemic significance. But Luick's theory is of importance in explaining why forms like *farende*, *farenne* are not to be considered analogical. See below, § 9.

[5] For the explanation of this point of view, see Luick, § 116, Anm. 2, § 161, Anm. 5, and § 164. See also the discussion of levelling in paradigms in § 162. Luick interprets plural froms like *hættas*, *cræftas*, *æc(e)ras*, *næglas* as later levelling from the singular.

This would suggest the following allophonic distribution immediately following the reversion of *æ* to *a* in some positions in the third or fourth century (that is, *after* Breaking, but *before* Diphthongization after Palatals, *i*-Umlaut, Smoothing and Second Brightening in Merc. and Kent.):

a) Monosyllables:

	Single Final Cons.	Geminate Final Cons.	Cons. Cluster
[æ]:	In all dialects before all C except N, *h*, or *w*: *bæc* 'back', *smæl* 'small', *fæt* 'cup', *fær* 'trip'.	In all dialects before all CC except NN, *rr*, *ll*, *hh*: *hætt* 'hat', *cnæpp* 'mountain top'.	In all dialects before all CC except NC, *r*C, *l*C, and *h*C: *fæst* 'fast', *cræft* 'strength'.
[a]:		In Nh. before *rr*, and general Angl. before *ll*: *darr* 'he dares', *all* 'all'.	In Nh. (and a few in Merc.) before *r*C and general Angl. before *l*C: *arm* 'arm', *ald* 'old.
'å' = [ɔ]:[6]	In all dialects before N: *nom nam* 'he took'.	In all dialects before NN: *mann monn* 'man'.	In all dialects before NC: *lamb lomb* 'lamb'.
'ea':	In all dialects before *h*: *seah* 'he saw'.	In all dialects before *hh*; in WS, Kent., and Merc. before *rr* and in WS and Kent. before *ll*: *dearr* 'dares', *eall* 'all'.	In all dialects before *h*C: *meaht* 'might'; in WS, some Merc. and Kent. before *r*C and in WS and Kent. before *l*C: *earm* 'arm', *eald* 'old'.

b) Polysyllables:

	Single Medial Cons.	Geminate Medial Cons.	Medial Cons. Cluster
[æ]:	In all dialects before C (except *h*, N, or *w* not followed by *i*) before front vowels or syllabic C: **mæli* (>*mete*) 'meat', **dæges* (>*dæges*) 'day's' (g. s.), *æcer* 'acre' (<**akra*), *fæðm* 'fathom', **nærian* 'to save'; in some Merc. not before *-læ-* (see below).	In all dialects before all consonants (except *ll*, *rr*, *hh*, NN) plus front vowels: *hættes* 'hat's'; in WS, Kent. and some Merc. before *ll* <**lj*: **læljan* 'to tell'.	In all dialects before all clusters (except NC, *l*C, *r*C, *h*C) before front vowels: *æfter* 'after', *fæste* (pl.) 'fast', *fægre* (pl.) 'fair'.
[a]:	In all dialects before all C (except N or *h*) plus back vowels:	In all dialects before all consonants (except *ll*, *rr*, *hh*, NN) before back vowels:	In all dialects before all clusters (except *l*C, *r*C, *h*C, NC) before back vowels:

[6] I take the orthographic vacillation of 'a' and 'o', marked 'å' in Luick, the reflex of Wgmc *a* before nasals, to indicate the existence of a slightly round back allophone [ɔ], best assigned to /æ/ (that is, until *a* becomes phonemic). I prefer this assignment, rather than to /o/, because the i-umlaut of 'å' is written *æ* in the earlier texts just as is the *i*-Umlaut of /a/ in its first stage. See the discussion of forms like *cæmpa*, *ænid* and *mængan* below, § 7 (b).

	hara 'hare', *sacu* 'strife', *sadol* 'saddle'; before *w* plus any following vowel except *i*: *clawe* 'claw' (d. s.); in some Merc. texts before *-læ-*(>*e*): *hwales* 'the whale's' but not before *i* or *j*.	*crabba* 'crab', *malluc* 'fork'; in all Angl. dialects before original *ll*: *fallan* 'to fall', in a few texts also before *rr*: *uarras* 'warts' (Leid.) and in some Angl. dialects before *ll*<**lj*: **falljan* (>*fællan*).	**craftas* (pl.) 'strengths', **naglas* 'nails', **fagrum* (d. pl.) 'fair'; in general Angl. before *l*C and *r*C: *armas* 'arms', *haldan* 'to hold'; and in some Anglian texts before *l*C plus *i*: **aldiro* (>*ældra* vs. *eldra*) 'elder'.
[ɒ]:	In all dialects before nasals: *nama noma* 'name', *hana hona* 'cock'.	In all dialects before nasals: *spannan sponnan* 'to attack.'	In all dialects before nasals: *standan* 'to stand'.
'ea'	In all dialects before *h*: **sleahan* (>*slēān*) 'to slay'.	In all dialects before *hh*: **hleahhjan* (>WS *hliehhan*) 'to laugh' (Angl. *hlæhhan* for *hlehhan* is analogical); and before *rr* in WS, some Merc., Kent.: *pearroc* 'park'; and WS, Kent., before *ll*: *feallan* 'to fall'.	In all dialects before *h*C: *eahla* 'eight'; in Ws, some Merc. and Kent. before *r*C, and in WS and Kent. before *l*C: *earmas* 'arms', *healdan* 'to hold'.

The problem is to find the environments which altered in such a way that [æ] and [a] could come into opposition. Since the present attempt is to discover the genesis of the opposition, one must look for the earliest such environmental alterations. Examining the environments presented in the chart above, the following formulation becomes evident.

4. THE MONOSYLLABIC ENVIRONMENTS: As far as monosyllables are concerned, they can be treated very briefly. An opposition-making situation could arise by sound change in one position, in Angl., namely where an original Gmc. geminate *l* (or *r* in Nh.) was simplified[7] putting *æ* and *a* into opposition before phonemically single *l* or *r*:

C *æ* *l*, *r*	C *æ* *l*, *r*
C *a* *ll*, *rr*	C *a* *l*, *r*

[7] This interpretation is based on H. Kurath, "The Loss of Long Consonants and the Rise of Voiced Fricatives in Middle English," *Language* XXXII (1956), 435: the graphic inconsistency in final geminate consonants in OE suggests to Kurath that "we may safely draw the inference that long consonants were no longer phonemic in this position." This would seem to replace the older view as represented by K. Bülbring, § 554 Anm. 1, "Im Wort- und Silbenauslaut wird häufig einfacher Konsonant statt der Gemination geschrieben; aber es ist darum doch keine lautliche Kürzung anzunehmen..." See further Sievers-Brunner, § 231.1, Wright, § 259.1,

Thus minimal pairs arose like the following: *stal* 'stall' (Cp.) vs. *smæl* 'small' (Ep.), *wal(l)* 'wall' (VP) vs. *wæl* 'battle' (Cp.), *hal(l)* 'hall' (VP) vs. **hæl* 'concealed' (VP has the brightened form *hel*); also Nh. forms in *rr: war(r)* (presumed n.s. form of *uarras*) 'wart' (Leid. Gl.) vs. *wær* 'wary' (Lind.), *far(r)* 'bull' (n. s. of *farras*, Lind.) vs. *fær* 'trip' (Lind.). This was obscured in later SW Merc. by the Second Brightening of *æ* to *a*.[8] Since the dating of the phonemic simplification of final geminate consonants is not certain, it is difficult to know when the opposition was fixed, but it is interesting to note forms like *stal* occurring as early as Cp.

5. The Polysyllabic Environment: Among polysyllabic words, *a* and *æ* may best be considered in three separate environments: before simple consonants ($\binom{a}{æ}$ C V), before consonant clusters ($\binom{a}{æ}$ C C V), and in trisyllabic words ($\binom{a}{æ}$ C (C) V C V). In each environment two events could have occurred to bring *a* and *æ* into opposition: either the unstressed V's in second syllables could have fallen together, thus

$$a\ \text{C}\ (\text{C})\ \text{V}_1 \rightarrow a\ \text{C}\ (\text{C})\ \text{V}_2$$
$$æ\ \text{C}\ (\text{C})\ \text{V}_2 \rightarrow æ\ \text{C}\ (\text{C})\ \text{V}_2$$

or the unstressed V's could have been lost, thus,

$$a\ \text{C}\ (\text{C})\ \text{V}_1 \rightarrow a\ \text{C}\ (\text{C})$$
$$æ\ \text{C}\ (\text{C})\ \text{V}_2 \rightarrow æ\ \text{C}\ (\text{C})$$

6. *I*-Umlaut (6th century) was the first major sound change after Brightening, and Darkening before Back Vowels, which could have effected the opposition. (The others, in Luick's chronology, were developments of Wgmc. *iu*, *eo*, *eu* and Diphthongization by Palatalization, which are clearly not relevant; the early dating of Second Brightening in VP and Kentish has been challenged by Sherman Kuhn, who takes it as an 8th or 9th century change[9]). Obviously, *i*-Umlaut itself does not explain the rise of phonemic oppositions—they could only occur where the umlauting *-i-* itself was either lost (after long syllables) or fell together with *e* (from

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Kieckers, § 34.3, and Luick-Wild-Koziol, § 625.2, § 644.1. A Celtic loan word which preserved *a* before obstruents as well was *bratt* 'cloak', which contrasted with *ræt* 'rat'.

[8] Luick, § 180; Sievers-Brunner, § 52; Bülbring, § 91 Anm. 1.

[9] Sherman Kuhn, "The Dialect of the Corpus Glossary", *PMLA* LIV (1939), 16. Luick's chronology of Second Brightening appears in § 181.

unstressed *æ*). The date for these developments is, respectively, early 7th century, and late 7th, early 8th century.[10]

The influence of *i*-Umlaut and changes of *i* in later syllables can be considered in terms of bisyllabic and trisyllabic words.

7. *I*-Umlaut in Bisyllabic Words

a) An opposition developed very clearly in Anglian where *a* became *æ* before *l*C followed by original *i/j*, while it was preserved before *l*C without following *i/j*, thus:

$$a\ l\mathrm{C}\ i/j\ \mathrm{V} \rightarrow æ\ l\mathrm{C}\ \mathrm{V}$$
$$a\ l\mathrm{C}\ \mathrm{V} \rightarrow a\ l\mathrm{C}\ \mathrm{V}$$

Thus arose clear-cut oppositions between *fællan* 'to fell' (Bülb. § 175) and *fæll* 'ruin' (Lind.) vs. *falla(n)* 'to fall' (Lind.), *wæll(e)* 'well' (VP) vs. *wall* 'wall' (VP). (A loanword in WS like *pæll* 'pall', from Latin *pallium*, may be explained as having been borrowed in the period when *i*-Umlaut could still operate but after Breaking ceased to be possible.) Oppositions of this sort before *l* plus other C occur as well: *cælf* 'calf' (Cp.) vs. *half* 'half' (Cp.), *cwæ lman* 'to kill' (VP) vs. *cwalm* 'death' (Cp. *utcwalm*), *bælg* 'bag, bellows' (Cp. *blæs tbælg*) vs. *balg* 'swelled with anger', *fælge* 'felloe' (Cp.) vs. *ælfalh* 'adhered' (VP) and *forswalg* 'swallowed up' (VP), *ældu* 'age' (VP) vs. *aldum* 'old' (d. pl.) (VP). To this might be added the Latin loanword *cælc* (<*calic*) 'chalice' vs. *calc* 'shoe' (<*calceus*). Similarly there are a few oppositions before *r*C, in Nh.: *cærra* 'to turn' (one occurrence in Lind.) vs. *carr* 'stone' (Lind.); *wærc* 'pain' (Lind.) vs. *arg* 'evil' (Lind.), *wærma* 'to warm' (Lind.) vs. *arm* 'arm' (Lind.), *hwærfa* 'to wend' (Lind.) vs. *hwarf* 'error' (Lind.).[11]

(b) Another important source of the opposition for the earlier period was the *i*-Umlaut of 'å' (see footnote 6). Since 'å' was not subject to Brightening, its *i*-Umlaut product was initially *æ*. Early texts (e.g., Ep.) have forms like *cæmpa* 'champion', *ænid* 'drake', *mængan* 'to mix'; the later development was to *e: cempa*, *ened*, *mengan*. Thus arose early oppositions like *cæmpa* vs. *camp-* 'battle' (Ep.) or *camb* 'comb' (Ep.), *cændæ* (Leid. Rid.)

[10] According to Luick, certain unstressed *e* and *æ* were first to be lost, § 303, and then certain unstressed *i* and *u*, § 304, § 309; other unstressed *æ*>*e* and *i*>*e* at the end of the 7th century or beginning of the 8th, § 324 and § 325. For the explanation of the role of geminating and umlauting *-j-*, its alternation with *-i-* after long syllables and its ultimate syncopation, see Sievers-Brunner, § 177.3 and § 177 Anm. 3.

[11] Luick, § 188.1; Bülbring, § 132, § 176; Sievers-Brunner, §§ 84-85.

'engendered' vs. (presumably) *hand* 'hand', *gimængdæ* 'he mixed' (Ep.) vs. *gegangendo* 'going' (Cp.). Later southern texts preserve *æ* through the whole OE period: *sændan* 'to send' vs. *sand* 'sand', *hænn* 'hen' vs. *cann* 'cognizance', *mænn* 'men' vs. *man* 'man', *þæncan* 'to think' vs. *þanc* 'thanks', *stæng* 'pole' vs. *tang* 'tongs', *lændenu* 'loins' vs. *lande* 'land' (d. s.).[12]

(c) Finally, some isolated forms did not become *e* through *i*-Umlaut but preserved *æ* instead.[13] The handbooks acknowledge the difficulty of explaining these forms, suggesting in some instances that analogy operated to change *æ* to *a* before the operation of *i*-Umlaut. Whatever the explanation may be, some forms with *æ* could occur before back vowels and come into opposition with *a* in these positions: *dwæscan* 'to choke' vs. *wascan* 'to wash', *mæslan* 'to fatten' vs. *gaslas* 'guests', *slæppan* 'to step' vs. *lappan* 'lappet' (d. s.), *wræcca* 'wretched', vs. *racca* 'part of ship-rigging', *mæcga* 'man' vs. **slacga* 'stag' and *Bacga* (proper name).

8. *I*-Umlaut could have effected an opposition-making environment in trisyllabic words if the *i* which appeared in the third syllable umlauted a second syllable vowel, which in turn fell in with the second syllable vowel of the resemblant form, that is:

$$æ\ C\ V_1\ C\ i \rightarrow æ\ C\ V_2\ C$$
$$a\ C\ V_2\ C\ \text{non-}i \rightarrow a\ C\ V_2\ C$$

The four medial vowels affected by umlaut are as follows: $u > y > i$ (**gadyling* > **gædiling* > **gædeling* 'kinsman'), $ō > ǭ > e$ (**hōcōdi* > **hōcǭdi* > *hōcede* 'curved'), $â > æ > e$ (**helpændi* > **helpendi* > *helpende* 'helping'), and $æ > e$ (**æþæli* > *æþele* 'noble').[14]

For the present purposes, only the umlaut of medial 'å' to *æ* to *e*, and of *o* to *e* are relevant since stressed *a* could only have occurred before original unstressed 'å' or *o*. (Before original unstressed *æ*, stressed *æ* was preserved; and the change of *u* to *i* cannot be included either because *y*, later *i* (finally *e*), the umlaut product of *u*, caused "double-umlaut" of the root syllable, thus: **harubist* > *hærfest* 'harvest', **gaduling* > *gædeling*, **gaduri* > *(æt)-gædere*, **latumist* > *lætemest*, etc.).[15]

[12] Luick, § 186; Bülbring, § 171; Sievers-Brunner, § 96.5. Anm. 8.

[13] Luick, § 189.2a; Bülbring, § 169; Sievers-Brunner, § 96.2 and § 96.3, and Stockwell-Barritt, p. 37.

[14] Luick, § 302.

[15] Luick, § 198a, § 198 Anm. 3; Bülbring, § 174; Kuhn, p. 17.

9. In the case of 'å', where the umlaut product is first *æ* and then *e*, distributions like the following could develop:

a C *å* N C *i*[16] > *a* C *æ* N C *i* > *a* C *e* N C *e*

(The change to *æ* is clear in the Ep. forms *lǣcnændi* 'showing', *cīnændi* 'splitting'.) This environment occurs in two forms in OE, the present participle *(-ende* < **-andi)*[17] and the inflected (dative) infinitive *(-enne* < **-anni)*[18] in Class VI strong verbs like *farende, lō farenne; bacende, lō bacenne; wacende, lō wacenne; dragende, lō dragenne; galende, lō galenne.* If we follow Luick's reasoning (see footnote 4 above), these forms must have had the following history: **farandi* > **færandi*[19] > **farandi* (by 'Darkening before Back Vowels') > **farændi* (by *ī*-umlaut) > *farende;*[20] **faranni* > **færanni* > **faranni* > **farænni* > *farenne.* Consequently the *a* in these forms is not analogical (as suggested, for example, by Stockwell-Barritt[21]), but the normal phonological development.

The inflected infinitive is particularly useful because these forms came into clear-cut opposition with adjectives ending in *-en* (from Pgmc *-īnaz*[22]) inflected for strong acc. sg. masc., thus: *wascenne* 'to wash' vs. *æscenne* 'ashen, made of ash-wood', *gnagenne* 'to gnaw', vs. *fægenne* 'fain', *farenne* 'to go' vs. *glærenne* 'vitreous', *hladenne* 'to lade' vs. *lædenne* 'Latin', *galenne* 'to sing' vs. *sælenne* 'made of willow'.

10. As far as oppositions developing from reflexes of **a* C *o* C *i* are concerned, these seem hard to find. One would suppose that the second class weak verbs would be a fruitful source. Their development in the 1st. sg. and pl. pres. ind., and inf., **-ōju* > **-eje* > *-i(j)e*, **-ōjanþi* > **-ejaþ* > *i(j)aþ*, and **-ōjan* > **-ejan* > *-i(j)an*,[23] did not umlaut a preceding *a*, so that a full range of forms such as *macige, maciaþ* and *macian* 'to make' could develop, cf. *baðian* 'to bathe', *warian* 'to guard', *þancian* 'to thank', *manian*

[16] 'å' can only occur before NC because unstressed 'â' had already become *æ* in open syllables by Brightening. Luick, § 301.3.

[17] Sievers-Brunner, § 363.3; Wright, § 479.

[18] Sievers-Brunner, § 363.2; Wright, § 480.

[19] Luick, § 301.2. Root vowel becomes *æ* by Brightening, but unstressed 'â' remains in closed syllables.

[20] Luick, § 302.3.

[21] Stockwell-Barritt, p. 38, fn. 8 (2).

[22] Wright, § 625.

[23] Luick, § 198 Anm. 5, Sievers-Brunner, § 411.1; Wright, § 535; Kieckers, § 130.

'to exhort', *wacian* 'to keep watch', etc. Unfortunately, although the list is extensive, it is difficult to find opposing *æ*-forms preserving *i* plus vowel; thus perhaps inflected forms of *-ig* adjectives,[24] e.g., WS *mænige* 'many' (str. acc. fem. sg.) vs. *manige* 'I admonish', *næfige* 'poor' (str. acc. sg. fem.) vs. *lafige* 'I wash'. But *æ*-forms are not numerous in *-ig* adjectives because *i* usually had already caused umlaut to *e*.

11. The above forms would seem to indicate that there is ample evidence for assuming that the *a*/*æ* opposition was established in a few environments by the 7th century when umlauting *i* was dropped or had changed to *e*, thus falling together with *e* from unstressed *æ*. There are a few other phonological developments which could contribute to the number of opposition-making environments; these can be listed summarily: (a) When unstressed *æ* and *e* were lost in some medial syllables,[25] oppositions could conceivably develop with those few *a*'s preserved before consonant clusters. Thus *scæfþa* 'chip' vs. *saftriende* 'rheumatic'. But examples are difficult to find. (b) The loss, in final position after long syllables, of unstressed *u* (7th cent.)[26] helped establish oppositions in a few positions, namely after geminate consonants which then simplified (as above § 4). But only a few Latin loanwords seem to be involved here. Examples are *batt* (from L. *battuere*) 'bat' and *catt* (from L. *cattus*) 'cat' vs. *fæt* 'cup' and *cræt* 'cart', *sacc* (from L. *saccus*) 'sack' vs. *sæcc* 'contention'.[27] (c) An eighth-century dissimilation in trisyllabic words[28] provided a number of opposing pairs in which middle syllable vowels were reduced to *e* before following back vowels. Thus *nafela* (Cp. *nabula*) 'navel', *adesa* (<*adosa*) 'adze', *afera* (<*afora*) 'child', *staðelode* (*staðol-*) 'made steady', *adela* 'filthy', *gaderian* (<**gaðurojan*), *stapelas* (sg. *stapol*), 'pillars', *hafela* (<*hafola*) 'head', etc. Thus, contrasts develop like *adela*, *staðelode* vs. *æðele*

[24] Wright, § 630.

[25] Luick, § 303; Sievers-Brunner, § 159; Wright, § 221.

[26] Luick, § 304b.

[27] But medial *u* was not lost after stressed short open syllables so there was no possible occasion for opposition-making environments to develop there. See Luick, § 306, Sievers-Brunner, § 146, Wright, § 221. Furthermore, svarabhakti vowel insertions were not opposition-making since the vowel inserted tended to be front after front vowels and back after back, thus *hægl*>*hægil* 'hail', while *tacn*>*tacun* 'token', see Luick § 317, Wright § 220.

[28] Called by Luick, § 347, "Jüngere Vokalabstufung" and by Sievers-Brunner, § 142, "Jüngerer Vokalwechsel"; cf. also Wright, § 222.

'noble', *hafela*, *nafela* vs. *gæfela* 'forks' (g. pl.) or *hæfena* 'havens' (g. pl.), *slapelas* vs. *slæpe* 'step', *gaderian* vs. *ælgædere* 'together', *afera* vs. *hæfera* 'of he-goats' (g. pl.), etc. d) Another source of *a* forms before *e* was Latin loanwords of the later period, insofar as *a* was not lengthened:[29] *casse* (L. *cassis*) 'hunting-net' vs. *mæsse* 'mass' (*casse* occurs only in the pl.), *castel* (L. *castellum*) 'castle' vs. *fæsten* 'fortress', *flaxe*, *flasce* (L. *vasculum*) 'flask' vs. *fæxe* 'dye' (d.s.).

12. To complete the picture, it is necessary to list certain analogical forms which are not of genetic importance, but which served to expand the yield of the *a*/*æ* opposition. *a* frequently occurred instead of *æ* in paradigms on the analogy of neighboring forms. Thus, (1) before zero, imperatives of Class VI strong verbs: *bac* 'bake' vs. *bæc* 'back', *far* 'go' vs. *fær* 'trip', *graf* 'dig' vs. *græf* 'grave', *sac* 'contend' vs. *sæcc* 'contention', *gal* 'sing' vs. *wæl* 'slaughter', *hlad* 'load' vs. *hlæd* 'mound'; (2) before *-e* in masc. and neuter instrumental sing. and fem. acc. sg. of strong adjectives and *e* adverbs: *glade* 'glad(ly)' vs. *hlædel* 'ladle', *bare* 'bare(ly)' vs. *fære* 'journey' (d.s.), *blace* 'black(ly)' vs. *blæce* 'ink' (d.s.), *hwate* 'brisk(ly)' vs. *wæter* 'water', *smale* 'small' vs. *dæle* 'dale' (d.s.); in oblique cases of some strong feminine nouns on the analogy of n.s.: *tale* 'tale' (d.s.) vs. *dæle* 'dale' (d.s.), *paþe* 'path' (d.s.) vs. *bæþe* 'bath' (d.s.), *wrace* 'revenge' (d.s.) vs. *bæce* 'back' (d.s.), *andsware* 'answer' (d.s.) vs. *spæren* 'gypsum', *nafe* 'nave of wheel' vs. *slæfe* 'staff' (d.s.), *sage* 'saw' (d.s.) vs. *dæge* 'day' (d.s.); (3) in past participles of strong Class VI verbs (and *habban*): *faren* 'gone' vs. *fære* 'journey' (d.s.), *grafen* 'dug' vs. *græfe* 'grave' (d.s.), *waden* 'gone' vs. *wæde* 'water' (d.s), *hafen* 'had' vs. *hæfen* 'haven'. Similarly in EWS and Kent. in 1st. sg. pres. ind. (instead of *u*, *o*)[30]: *ic grafe*, *ic wade* vs. *græfe*, *wæde*, etc.; (4) in the g. pl. of *n*-stems, the following development is usually hypothesized: **-ana* > **-æna* > *-ena*.[31] The gen. pl. originally had zero-grade suffix; the suffix *-a-* was introduced by analogy with sg. and nom. pl. in the fem. forms which were later generalized in the masc. as well. By Luick's argument (see footnote 4 above), the development of typical *n*-stem g. pl. forms can be assumed to have

[29] Examples of lengthened forms are *sācerd*, *pālendse*, *drācentse*, *cālend*, *cāsere*, etc. See Luick, § 104 and § 218.2.

[30] Wright, § 476.

[31] Wright, § 400; E. Prokosch, *A Comparative Germanic Grammar*, (Philadelphia: Linguistic Society, 1939), § 84d.

been as follows: **hacana* > **hæcæna*.[32] This would have given historically **hæcena;* thus we must interpret *hacena* as analogical. From such forms arose oppositions like *hacena* 'of the hooks' vs. *hæcena* 'of the gratings', *assena* 'of the asses' vs. *mæssena* 'of the masses', *cnapena* 'of the boys' vs. *cæppena* 'of the caps', *cnafena* 'of the boys' vs. *hæfena* 'of the havens', *crabbena* 'of the crabs' vs. *hæbbe* 'I have', *harena* 'of the hares' vs. *bærena* 'bare' (inflected for g.pl. weak), *magena* 'of the stomachs' vs. *mægena* 'of strength' (g.pl.), *ascena* 'of the ashes' vs. *æscena* 'of ash-wood vessals', *faðena* 'of the aunts' vs. *flæþecomb* 'weaver's comb', *gadena* 'of comrades' vs. *gæde* 'society' (d.s.).

University of Pennsylvania.

[32] Luick, § 301.3. That is, since unstressed *a* > *æ* in open syllables, *æ* is preserved in the root syllable.

THE COLLOQUIAL PRETERITE IN MODERN AMERICAN ENGLISH

GERARD VANNECK

The distinction between preterite and perfect is one that many Indo-European languages have alternately lost and reintroduced at various times in their histories. Thus, though Latin and Gothic had only one tense to do duty for both aorist and perfect, the Romance and modern Germanic languages subsequently developed a new compound tense to supply much the same shade of meaning as the original (e. g. Greek) perfect. Since then, modern spoken French and several Germanic languages (e.g. Swiss German, Yiddish, Afrikaans) have once more ceased to distinguish between preterite and perfect.

There are signs that in modern spoken American English, too, the distinction between preterite and perfect is beginning to be lost, though the present tendency may be checked by the influence of the written language. Very many speakers no longer feel any instinctive need for the perfect tense in a number of contexts which traditionally require it. Such speakers habitually replace it by what I propose to call "the colloquial preterite," though this usage is studiously avoided in writing.

The following paper gives a brief description of the present situation in this respect, as it appears to a British observer. Not being a philologist, I have made no attempt at a modern scientific presentation, but have expressed myself in terms of elementary school grammar. The earlier part (paragraphs 1-10) describes the phenomena I have observed; the latter part (paragraphs 11-18) contains some speculations as to their causes and future development. The subject matter is the everyday speech of educated native Americans: my observations have all been taken from, or carefully checked with, university graduates born and educated in the United States.

1. Written British English, spoken British English and written American English agree closely in their choice between the preterite

and the perfect. Spoken American English, however, uses the preterite in many cases where British English and written American English use the perfect. Examples:

(a) He isn't there now. I don't know what happened to him.

(b) Spain's a nice country. I know some people who were there.

(c) Hurray! He did it again!

(d) Darn it! I did it again!

(e) Yes, he's here. I just saw him.

(f) You missed him. He just went out.

(g) Did you have lunch (yet, already)?

(h) I didn't pay for this book yet.

(i) That show's still on. I saw it twice.

(j) I live in New York, but I never saw the St. Patrick's Day Parade.

I shall call these uses of the preterite "the colloquial preterite," the adjectives "modern American" being understood.

2. The colloquial preterite is a genuine preterite, not a perfect with the auxiliary slurred or omitted. The verbs in most of the above examples have forms which make this clear. Such verbs are few in number, but very frequently used. Most verbs, when not used negatively or interrogatively, admit of either interpretation. Examples:

(k) You said it, brother!

(l) I finished. Let's go eat.

3. In all the above examples the perfect is an acceptable alternative in spoken American English; and some Americans habitually use it in nearly all of them. However, the perfect strikes me as being less common than the preterite in most of these examples and much less common in a few of them, namely in examples (a), (c), (d), (f) and (k). But of course I am less likely to notice the tense that I also use myself.

4. In general, there are far fewer cases in spoken British English than in spoken American English where the preterite and the perfect can be used interchangeably. In spoken British English, the use of the preterite instead of the perfect clearly implies a different kind of context. Examples:

(m) You look worried. What's happened? (I've no idea what my friend may have seen or heard.)

(n) You look worried. What happened? (I knew that my friend was to attend a meeting, which is now over. I want to know what happened at it.)

(o) I've just seen him. (I've seen him very recently.)

(p) I just saw him. (I saw him, but I didn't speak to him. This may refer to any time in the past.)

(q) Have you had lunch? (If not, would you care to join me?)

(r) Did you have lunch? (If not, you must have been very hungry, unless you managed to get a snack later.)

(s) He's never seen the St. Patrick's Day Parade. (But perhaps he'll see it this year.)

(t) He never saw the St. Patrick's Day Parade. (During all the years he lived in New York he never saw it; and now, as you know, he lives in Paris.)

These shades of meaning are obliterated in the examples quoted in paragraphs 1 and 2 and are presumably ignored by the speakers from whom those examples were taken. If required, such shades of meaning can, of course, be expressed in some other way.

5. The colloquial preterite may be related to the equally common American use of the preterite in contexts which seem to demand the pluperfect. This, however, is by no means confined to the spoken language. It also occurs, though less frequently, in British English. Examples:

(u) When I saw her, she didn't have the baby yet.

(v) I saw you here last week. You were looking kind of lost. Did you just arrive?

(w) When some disaster would hit us, he would say God was punishing us for something bad we did.

These three examples would not occur in British English or in written American English.

6. The many educated Americans who use the colloquial preterite nearly always replace it with the perfect when writing or when delivering a formal speech. Some of them believe that they do not use it in cases where, in fact, they do. Many Americans are unaware of the existence of the colloquial preterite; perhaps they are so used to hearing it (and using it) that they never notice it and are thus free to believe that it does not occur.

7. Curiously enough, most Americans have very little recollection

of being taught that the preterite in such cases is incorrect or slang, though that is how some of them describe it. One or two teachers have told me that they condemn the colloquial preterite on logical grounds; others say they have no fault to find with it, though the perfect might be more elegant.

8. Very few Americans are aware that their use of the perfect differs from that of the British. This follows naturally from paragraph 3. (The slang phrase "You've had it", "He's had it" etc. was recently imported from England without any change of tense; whereas the American phrase "You said it!" became "You've said it!" in England.)

9. The above may be summed up by saying that in many contexts Americans spontaneously use the preterite instead of the perfect, which is obscurely felt to be a somewhat literary tense.

10. A natural corollary is the hypercorrect use of the perfect in written American English in contexts where the preterite would be preferable, and sometimes where it is obligatory. Examples:

(x) "Isak Dinesen has, of course, a long time ago made herself master of the tale..." (article by Eudora Welty in the *New York Times Book Review*)

(y) "..., as I have predicted to you two years ago, ..." (*Mission to Moscow* by Joseph E. Davies, quoted by Edmund Wilson in *A Literary Chronical: 1920-1950*)

Such examples, which are not frequent, constitute a slight exception to the usual agreement between written American and British English with regard to the use of the perfect.

11. Though the colloquial preterite is widespread in all classes of American society, it is not mentioned either in Curme's *Syntax* (1931) or in Mencken's *The American Language*. Mencken described the occurrence of incorrect forms like "I taken" and "I have took," but not the "incorrect" use of correctly formed preterites. This suggests that the colloquial preterite was less prevalent in his day. I am assuming that the colloquial preterite represents a new development, and not a survival. This hypothesis is supported by the facts mentioned in paragraph 15 below, but it needs to be tested by research.

12. Another subject for research is the relation between the colloquial preterite of today and the perfect without an auxiliary (e.g. "I taken"), which was once an outstanding feature of uneducated

speech. The colloquial preterite might have arisen in one or more of the following ways:

(a) as a direct continuation of the perfect without an auxiliary, assimilated to the preterite;

(b) as a reaction against the perfect without an auxiliary, among people who had been made ashamed of it and who finally avoided all spoken forms of the perfect, in order to be on the safe side;

(c) as a similar reaction against the perfect without an auxiliary, but among more educated people, who had never used it themselves; or

(d) as an entirely independent development, due to other causes, among people who had never used the perfect without an auxiliary.

13. The colloquial preterite is certainly not confined to areas of recent massive immigration, like New York. But non-British immigrant influence must be considered as a possible contributory factor. Many immigrants would naturally tend to use the perfect, rather than the preterite (see paragraph 16); of these, some might conscientiously overdo the preterite. The great majority of non-British immigrants, however, have this in common, that they are not used to differentiating between the perfect and the preterite in their own spoken mother tongue. Another important influence is that of Irish English, which may well prove to be the original source of the American colloquial preterite, since a similar use of the preterite is very common in Ireland.

14. Changes in the use of the perfect are facilitated by the inability of most educated English-speakers, whether American or British, to explain their choice between the perfect and the preterite. This is equally true of people whose usage is confident and correct, and even of many persons with some training in grammar. In fact, the meaning of the perfect is less clearly understood by English-speakers than that of any other tense.

15. Paragraph 14 seems to suggest the possibility that, even in Britain, the use of the perfect no longer represents a genuine desire for a certain shade of meaning, and that it is merely retained as a traditional idiomatic pattern, like the intricacies of "shall" and "will" in the future tense. This may be true of American English. But it can scarcely be true of British English, in view of the distinctions demonstrated in paragraph 4. Moreover, the

perfect is firmly rooted in uneducated British speech. In Cockney, for instance, the perfect is often emphasized by the addition of expletive verbs: where the American says "Darn it! I did it again!", the Cockney will say "Blimey! I've (been and) gone and done it again!"

16. The relations between the preterite and the perfect in spoken and written American English, respectively, are the reverse of the corresponding relations between these two tenses in Modern French. In fact, spoken American English is unusual among modern languages in preferring the preterite. Not only modern spoken French, but Russian and many others have chosen the perfect, which is more in keeping with the analytical tendency of modern languages and is often described as more "lively," i.e. more intimately connected with the present. The reason for the Americans' preference may perhaps lie in the rule forbidding the use of the English perfect in connection with any period of time that does not extend up to the present moment: the English-speaker cannot say, as so many non-English-speakers can, "Yesterday I have finally succeeded." Most uses of the perfect in English consequently suffer from an inherent chronological vagueness, which makes the tense seem less objectively vivid than the preterite, except when applied to the very recent past.

17. It is evident from paragraphs 6 and 10 that there is some reaction against the colloquial preterite. But, even if all schoolteachers dedicated themselves to the restoration of the perfect, their task would still be a hard one; for the tense is disappearing from precisely the most living part of the language: simple, everyday remarks, spontaneous exclamations etc.

18. It remains to be seen whether a wholesale abandonment of the perfect (and pluperfect) is under way in spoken American English. In many other languages no need is felt to distinguish between the perfect and the preterite. And, after all, the exceptionally rich tense-system of modern English is unlikely to be preserved for ever. Indeed, if there were no written language, we might well be witnessing the first stages in the loss of the perfect tense. As it is, the worst that can be expected is a progressive widening of the present divergence between the spoken and the written language and a more frequent abuse of the perfect by people wishing to improve their written style.

New York City.

SYSTEMIC SUPPORT FOR THE *x*/*ç* DISTINCTION IN GERMAN

PAVEL TROST

In Modern Standard German, the *ach-Laut* [x] and the *Ich-Laut* [ç] are clearly distinguished. Whether these two sounds are to be considered separate phonemes or allophones of a single phoneme depends entirely on whether one admits phonically unmarked morpheme boundaries into the phonemic description;[1] i.e., *lauchen* [tauxen] 'to dive' and *Tauchen* [tauçan] 'little rope' can be distinguished either by setting up separate phonemes, /x/ and /ç/, or by stating that /x/ has an allophone [ç] which occurs (among others) after morpheme boundaries. Be that as it may, German orthoepy is strict in its requirement of a distinction. The *Bühnenaussprache* demands the [ç] sound (1) after front vowels, (2) after *r*, *l*, and *n*, and (3) in the suffix *-chen*, regardless of the preceding vowel; also, (4) in loanwords, initially before front vowels. Elsewhere, the [x] is obligatory.[2] Among the deviations specifically condemned are: (1) substitution of [x] for [ç], (2) use of a "middle x" between the *ich*-sound and the *ach*-sound, and (3) replacement of the *ich*-sound by *š*. However, in spite of Siebs, *ich*-sound and *ach*-sound are not distinguished throughout German language territory even in cultivated pronunciation. In Southern Germany, the same consonant [x] is used after both front and back vowels.

We raise the question whether the *x*/*ç* distinction is supported by the rest of the sound system of the several varieties of German;

[1] Cf., for example, Eduard Hermann, "Phonologische Mehrgültigkeit eines Lautes," *Philologische Wochenschrift* (1932), 115-117 (two phonemes); William G. Moulton, "Juncture in Modern Standard German," *Language* XXIII (1947), 212-226 (one phoneme); Werner F. Leopold, "German *ch*," *Language* XXIV (1948), 179f. (two phonemes); G. Dietrich, "[ç] und [x] im Deutschen — ein Phonem oder zwei?" *Zeitschrift für Phonetik und allgemeine Sprachwissenschaft* VI (1952), 28-37 (one phoneme).

[2] Th. Siebs, *Deutche Hochsprache (Bühnensprache)*, 16th ed., [Berlin 1957], p. 73.

that is to say, whether the observance or neglect of the distinction in various parts of German territory is a matter of accidental articulatory habits, or due to reasons of linguistic structure.

The *x*/*ç* distinction appears to be observed in Standard German in those parts of the country where it functions in colloquial speech *(Umgangssprache)* and, in some areas, in the dialects. Within the dialects, we observe that the *x*/*ç* distinction generally operates where [γ] and [j] are in complementary distribution.

The *ich*-sound is distinguished from the *ach*-sound, for instance, in Ripuarian.[3] Here a medial γ exists partly in complementary distribution with *j* (*krijə* 'kriegen', *klāγə* 'klagen'). However, while γ occurs only in medial position, *j* can be either medial or initial. Historically, in Ripuarian *g-*>*j-*, *-g-*>*-*γ*-* after back vowel and >*-j-* or removed after front vowel. In final position, *-g*, -γ are unvoiced to *-x* or *-ç*. Thus, the complementary distribution of γ and *j* is similar to that of *x* and *ç*.

The *ich*-sound was also distinguished from the *ach*-sound in the colloquial German of the Baltic provinces.[4] Here also a medial γ existed in complementary distribution with *j* (*lsījlər* 'Ziegler', *wāγnər* 'Wagner'). Phonemic analysis of Baltic German, based on Prague School principles, declared γ to be an allophone of *g*, but *j* a separate phoneme; on the other hand, the pairs *x*: γ and χ : *j* were ranged in a series of correlated voiceless and voiced phonemes. This is a self-contradictory result, reflecting the fact that medial γ of Baltic German was complementary both with *j* (in medial position) and with *g* (in initial position). As a result, γ cannot be identified with *g* on account of its relation to *j*, nor with *j* on account of its relation to *g*.

The distinction of *ich*-sound and *ach*-sound was found in Silesian, too.[5] Here also a medial γ existed in complementary distribution with *j* (*gehējə* 'Gehege', *auγa* 'Augen' and both sounds were here in complementary distribution with initial *g*. In Eastern Bohemia, German dialects (of a Silesian type) do not make a marked distinction between [x] and [ç], except in one small region—which, remarkably, is also the only one where γ is in complementary distribution with *j*.[6]

[3] Th. Frings, *Dialektgeographie des Niederrheins*, Marburg 1913, p. 26.

[4] G. Deeters, "Phonologische Bemerkungen zum baltischen Deutsch", *Travaux du Cercle linguistique de Prague* VIII (1939), 130-137, p. 136f.

[5] W. v. Unwerth, *Die Schlesische Mundart*, Breslau 1908, p. 53.

[6] F. Festa, *Die schlesische Mundart Ostböhmens*, Prague, 1926, p. 89.

The distinction of *ich*-sound and *ach*-sound is also found with certainty in Thuringian and Upper Saxonian, where in many districts, a counterpart distinction among the voiced consonants is not observable at present: medial γ and medial *j* do not occur at all.[7] But formerly -γ- and -*j*- did exist. Today, in the colloquial speech of Saxonian cities the *ich*-sound passes into, and merges with, *š*.[8] This change affects both *x* after front vowel and the [ç] which has resulted from the unvoicing of initial *j*- in a southern Saxon dialectal zone. After the passage of initial *j* into the *ich*-sound, we find it in complementary distribution with [x]. The new shift, now in progress, is establishing a new distinctive opposition, /š/∾/x/, which also exists in Standard German (though the *š* may occur in different words).

The same shift of [ç] into [š] is recorded in different parts of German territory. The phenomenon has been attributed to a strong tendency to palatalization (which in turn, taken as a hereditary peculiarity, served as a clue to the common origin of speakers in different places).[9] But such "articulatory tendencies" are irrelevant to linguistics, even if they are not legendary in themselves. No sound systems as a whole can be reduced to a definite articulatory tendency; for instance, there are no "palatal languages," for front vowels universally coexist with back vowels, palatalized consonants with plain consonants, etc. The passage of [ç] to [š] in colloquial or dialectal German constitutes a reduction of the phoneme inventory, not an " articulatory tendency."

It might be objected that the differentiation of [x] and [ç] was simply the consequence of the marked effect of palatal vowels on the following "guttural" consonants, voiced and unvoiced. Would not that split of "gutturals" be a mere phonetic fact? It is not a universal process; therefore it must not be taken as a "mere phonetic" fact.

German inherited the "guttural" *g* and the palatal *j*, which were opposed to each other as a plosive and a fricative consonant. When in some dialects stops in medial position came to be realized as fricatives, the opposition *g* : *j* was transformed into γ : *j*. Thus

[7] V. Kürsten and O. Bremer, *Lautlehre der Mundart von Bultelstedt*, Leipsic, 1910, p. 185ff.

[8] R. Grosse, "Leipzigisch *escha*," *Beiträge zur Geschichte der deutschen Sprache und Literatur* LXXIX (1957), 181-190.

[9] E. Schwarz, *Sudetendeutsche Sprachräume*, Munich 1935, p. 167.

in the course of German sound history *j* either physically changed into [g], or became distributionally a member of the "g" phoneme. The differentiation of [x] and [ɣ] resulted from the relation of *g* and *j* in the German consonant system.

Charles University, Prague.

TRANSFORM ANALYSIS OF RUSSIAN INSTRUMENTAL CONSTRUCTIONS

DEAN STODDARD WORTH

0. INTRODUCTION. The traditional approach to the syntactic description of standard Russian has been based on the morphological definition of word classes and consisted primarily of a more or less exhaustive listing of the various types of word-combination *(slovosočetanie)* and sentence into which members of these classes can be combined, e.g. substantive in various cases modifying a substantive, modifying a verb, etc. Such morphologically defined phrases are tacitly assumed to be the smallest formally characterized class above the word level. The morphological description by itself, however, produces an obviously superficial picture of Russian syntax, since there are in most cases from a few to many intuitively recognized different kinds of relation expressed within one and the same morphologically defined phrase type, e.g. in English the difference between "John was eating all the cheese" and "John was eating all the time," or in Russian the following sets of verb+substantive in the instrumental case: *rukovodit batal'onom*[1] 'is in charge of a batallion', *mašet platkom* 'waves his kerchief', *priezžael starikom* 'arrives an old man', *voet šakalom* 'howls like a jackal', *čitael večerom* 'reads in the evening', *idet lesom* 'walks through the forest', *govorit šopotom* 'talks in a whisper'. With the concept of form thus restricted to that of morphological description, one is faced by a multiplicity of meanings.

[1] Russian forms are given in the standard transliteration. The English translations are given as an aid to readers unfamiliar with Russian, but since, like all translations, they compromise between literalness and literacy, the reader is hereby warned against interpreting Russian syntactic structure on the basis of English translations. For example, one cannot equate the active—passive transformation in English with the $S^1\ V\ S^2 \rightarrow S^2\ V_{pass}\ S^1$ (see footnote 31 for symbols) transformation of Russian, since the latter turn out to be genuine passives only in a minority of cases (for a traditionally couched but penetrating discussion of this problem, see V. V. Vinogradov, *Russkij jazyk*, Moskva-Leningrad, 1947, pp. 629-641).

expressed by a single form, and has only the choice between (1) relegating all differences among units of like morphological structure to the realm of the lexicon and thus (assuming the lexicon has nothing to do with grammar) considering these differences none of the linguist's concern,[2] and (2) attempting to account for the intuitively recognized relational varieties within the morphologically defined class by dividing the latter into subclasses on a purely semantic basis. The latter solution is adopted, for example, by the latest full syntactic treatment of Russian, the second volume of the new grammar of the Soviet Academy.[3] For purposes of comparison we shall first outline the treatment of Russian instrumental constructions in this work.

0.1. Traditional Analysis. The Soviet Academy grammar divides word-combinations of verb and instrumental substantive modifier into five major classes (one of which is as a matter of fact already archaic, cf. below), on the basis of the kind of relation expressed between verb and substantive. These five classes are labeled objective, temporal, spatial, determinative-circumstantial, and causative; most of them are divided into a number of subclasses determined by a variety of criteria, mostly semantic. The largest of the five major classes, in which objective relations are expressed, is defined as expressing "an action and the instrument by means of which this action is accomplished,"[4] e.g. *rubit' toporom* 'chop with an axe', *pisat' černilami* 'write in ink'. A subclass contains verbs "with the meaning of allotment, equipment,

[2] The work that goes the farthest in the direction of eliminating nonformal categories is M. N. Peterson's concise *Sintaksis russkogo jazyka*, Moskva, 1930.

[3] Akademija Nauk SSSR, Institut jazykoznanija, *Grammatika russkogo jazyka, II, Sintaksis*, parts 1-2, Moskva, 1954. The most important earlier works with the same general approach are A. A. Šaxmatov, *Sintaksis russkogo jazyka*, 2nd ed., Leningrad, 1941, and A. M. Peškovskij, *Russkij sintaksis v naučnom osveščenii*, 7th ed., Moskva, 1956. The latest syntactic works repeat on a smaller scale the same semantic approach, e. g. E. M. Galkina-Fedoruk, ed., *Sovremennyj russkij jazyk. Sintaksis*, Moskva, 1957, A. N. Gvozdev, *Sovremennyj russkij literaturnyj jazyk. Čast' II. Sintaksis*, Moskva, 1958. The Academy grammar discusses instrumental constructions in different sections: as word-combinations, i. e. *slovosočetanija* (*Grammatika russkogo jazyka*, II, 1, 132ff.), as parts of sentences *(predloženija)*, especially after copulative and semi-copulative verbs such as *byt'* 'be', *kazat'sja* 'seem', *stat'* 'become', etc. (*op. cit.*, II, 1, p. 427ff.), and as various kinds of circumstantial description, i. e. *obstojatel'stvo* (*op. cit.*, II, 1, p. 572ff.); some constructions appear in more than one section, e. g. *rasstalis' soldatami* 'they parted as soldiers' (p. 137), *rasstalis' bol'šimi prijateljami* '(we) parted great friends' (p. 433).

[4] *Grammatika russkoga jazyka*, II, 1, 132.

provision in the broad sense" and substantives "signifying the object with which someone is provided or not provided,"[5] e.g. *nagradit' ordenom* 'confer a decoration', *snabdit' den'gami* 'provide with money', *obdelit' nasledstvom* 'deprive of an inheritance'. Another subclass contains verbs which "name a movement" and substantives which name "a part of the body or an object organically connected with the actor,"[6] e.g. *maxat' rukoj* 'wave one's arm', *topat' nogami* 'stamp one's feet'. Should verb and substantive be of more abstract meaning, they form "combinations, in which in the dependent word (= modifier) the meaning of instrument is somewhat weakened and is replaced by the more general meaning of indirect object,"[7] e.g. *udivit' umom* 'astonish by one's wit', *ugrožat' vojnoj* 'threaten with war', *umorit' golodom* 'starve (someone) to death' ('to kill by hunger'). Combinations expressing the relation labeled "indirect object" are themselves divided into a number of subgroups, the first of which contains verbs "signifying filling, satiation" and substantives naming "the object with which something is filled,"[8] e.g. *nabit' senom* 'stuff with hay', *ispolnit'sja nenavist'ju* 'become filled with hate', *nagruzit' poručenijami* 'burden with errands'; this subgroup is stated to contain words of both abstract and concrete meanings, which appears to contradict the subclass definition above.

A special paragraph is accorded those indirect object combinations in which the verb means "possession, internal enthusiasm, constant occupation,"[9] e.g. *vladet' francuzskim jazykom* 'speak French', *vostorgat'sja druz'jami* 'be delighted with one's friends', *zanimat'sja sportom* 'engage in sport', *ljubovat'sja prirodoj* 'admire nature'; in some cases, the instrumental substantive may at the same time name "the source of the feeling or experience expressed by the verb,"[10] e.g. *gordit'sja pobedoj* 'be proud of a victory', *plenjat'sja krasotoj* 'be captivated by beauty'. A further subclass (presumably still, but not explicitly stated as, expressing indirect objects) consists of substantives "upon which an activity is spent" and one of the seven verbs *vedat'* 'manage', *zavedovat'* idem,

[5] *Op. cit.*, 133.
[6] *Loc. cit.*
[7] *Op. cit.*, 134.
[8] *Loc. cit.*
[9] *Op. cit.*, 135. The semantic unity of this group is not as obvious to this writer as it was to the compilers of this grammar.
[10] *Loc. cit.* It is not clear how one is to distinguish between this type of combination and those of the fifth major class, labeled causative.

komandovat' 'command', *pravit'* 'rule, govern', *rasporjažat'sja* 'deal with, dispose of', *rukovodit'* 'direct', *upravljat'* 'govern'. The final subclass of the class of objective relations consists of combinations formed with either "a verb in the form of the passive voice" or "a passive participle" combined with an instrumental substantive which "names the producer of the action—a person or thing,"[11] e.g. *Činy ljud'mi dajutsja* 'ranks are given by people' (Griboedov), *Vražda umirilsja vlijan'em godov* '(Your) enmity will be calmed by the influence of the years' (Nekrasov), *Vse pokrylo bylo snegom* 'Everything was covered by snow' (Puškin), *Vse zdes' sozdano nami* 'Everything here has been created by us' (Nikolaeva). The awkwardness of including these obviously passive transforms in the objective class is apparently conceded by the remark that "in these cases the forms of combination are closely connected with the structure of so-called passive constructions and of a particular type of verbal sentence."[12]

The second major class in the Academy grammar consists of combinations expressing temporal relations. This class is divided into two subclasses, this time by purely morphological criteria. The first subclass contains substantives in the instrumental singular designating time of day or season of the year, and obligatorily accompanied by agreeing adjective or governed substantive modifiers, e.g. *on uexal glubokoj osen'ju* 'he left at the very end of autumn' ('in deep autumn'), *slučilos' pozdnej noč'ju* '(it) happened late in the night'.[13] A subgroup contains substantives

[11] *Op. cit.*, 136.

[12] All such combinations of passive participle and instrumental substantive are perfectly straightforward transforms of one of two types of phrase: (1) nominative substantive+transitive verb+accusative substantive, e. g. *Vse zdes' sozdano nami* 'Everything here has been created by us' ← *My sozdali vse zdes'* 'We created everything here'; (2) phrases already containing instrumental substantives and discussed elsewhere in this paper, e. g., *kniga, prikrytaja trjapočkoj* 'the book, covered by a rag' ← *X prikryl knigu trjapočkoj* 'X covered the book with a rag'. The first of these two transformation types corresponds exactly to the active—passive transformation in English (on the latter, see Noam Chomsky, *Syntactic Structures*, The Hague [1957], pp. 77ff.; Robert B. Lees, review of Chomsky, *Language* 33.375-408 [1957], esp. 388, and Zellig S. Harris, "Co-occurrence and Transformation in Linguistic Structure," *Language* 33.283-340 [1957], esp. 325ff.; further literature will be found in these works) and differs from the second just as "The wine was drunk by the guests" differs from "John was drunk by midnight" (examples from Chomsky, 80).

[13] Soviet linguists consider all such words to be adverbs when they occur without modifiers, e. g. *zimoj* 'in winter', *noč'ju* 'at night'. However, there seems to be no good reason for considering forms such as +*osen'ju*, +*večerom*, etc. to be adverbs but the second half of the forms *glubokoj*+*osen'ju*, *pozdnim*+*večerom* to be substan-

(animate, although this is not mentioned in the grammar) naming age, occupation, or social status in which the subject of the verb is placed at the time of the action, e.g. *on uexal rebenkom* 'he left a child' ('was a child when he left'), *rasstalis' soldatami, a vstretilis' polkovnikami* 'they parted as (simple) soldiers, and met (again) as colonels'; that this subgroup does not belong here is proved by the fact that it not only does not have to have, but in fact almost never does have, an adjective or substantive modifier of the instrumental substantive. The second subclass of temporal combinations contains substantives in the instrumental plural, which "name an action, repeated from time to time and lasting throughout the course of the period of time named by the substantive,"[14] e.g. *Aleksej celymi dnjami prigljadyvalsja k Komissaru;* it is not clear just what is different in this second subclass, apart from the plural morphemes and their meaning.

The third major class consists of combinations expressing spatial relations. These contain "a verb, signifying motion (and) a substantive in the instrumental naming a place, a space, along which the motion is directed,"[15] e.g. *probralsja ogorodami* 'he made his way through the back gardens', *exal lesom* 'he was riding through the forest'. Should the verb be other than a verb of motion, "the combination expressing spatial relations takes on the nuance of a temporal meaning,"[16] e.g. *Dorogoju stali bit'* 'Along the way they began to beat (him)' (Šoloxov).

The fourth major class, in which determinative-circumstantial *(opredelitel'no-obstojatel'stvennye)* relations are expressed, contains substantives which "name the mode *(sposob)* of accomplishment of the action named by the verb,"[17] e.g. *zapel vysočajšim fal'cetom*

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tives, since these forms occur in identical environments and one is always free to add or subtract the adjective modifier (transformations of the type T: ø → F, T: F → ø, cf. 0.221). In our opinion they are obviously a special subclass of substantives, formally characterized by (1) the fact that they can modify in the accusative case non-transitive verbs in *-sja*, e.g. *on oldyxalsja vsju zimu* 'he rested all winter long' and (2) in certain environments they can be modified only by a limited number of quantifying adjectives (restrictions on the transformation T: ø → A, cf. 0.221), e.g. *on vernulsja pozdnej osen'ju* 'he returned in late autumn' cannot → **on vernulsja pozdnej, xolodnoj, no vse-taki dovol'no prijatnoj osen'ju* 'he returned at a late, cold, but nonetheless rather pleasant time of autumn' (note that the English restrictions rather parallel the Russian).

[14] *Grammatika russkogo jazyka*, II, 1, 138.

[15] *Loc. cit.*

[16] *Loc. cit.*, fn.

[17] *Op. cit.*, 138-39.

'began to sing in a very high falsetto' (Turgenev), *Tanki goreli golubym plamenem* 'The tanks were burning in blue flame' (Ketlinskaja).[18] A subclass contains substantives which "signify the mode of completion of the action, appearing for the sake of comparison;"[19] here the grammar makes one of its few tentative steps toward the analytic use of transformations, e.g. *lečet rekoj* is compared with *lečet, kak reka* 'flows like a river'. In another subclass, the substantive "can characterize the mode of completion of the action from the quantitative side,"[20] e.g. *letjat stadami pticy* 'in flocks fly the birds' (Krylov), *kolorye sypal on meškami* 'which he poured by (whole) sacks' (Gogol'). Only a note mentions a particular type of determinative-circumstantial combination in which "the dependent substantive is by its lexical meaning close to the meaning of the governing verb,"[21] e.g. *izučajuščim vzgljadom ogljadel* 'looked about with a studying glance' (Ketlinskaja), *Bystrymi šagami ona šla* 'With quick steps she went' (Nikolaeva).[22]

The fifth major class, expressing causative relations, contains substantives which "signify a manifestation or state which has conditioned the action named by the verb."[23] Only two archaic examples are given, *Osel moj glupost'ju v poslovicu vošel* 'My donkey by (his) stupidity has got into the fable' (Krylov), and *Slučalos' li, čtob vy... Ošibkoju dobro o kom-nibud' skazali?* 'Has it occurred that you... by mistake said (some) good of someone?' (Griboedov); it is noted that such combinations are being replaced in modern Russian by constructions with *po* and the dative or *iz-za* or *ot* and the genitive, e.g. *sdelat' po ošibke* 'do by mistake', *otstaet iz-za leni, ot nevnimanija* 'lags behind because of laziness, from inattention'.[24]

0.2. Transformation Analysis. The haphazard quality of the traditional classification outlined above is obvious. The

[18] The grammar does not mention the essential structural fact that in almost all such combinations there is an obligatory adjective modifier of the instrumental substantive. Note for example the impossibility of **tanki goreli plamenem* 'the tanks were burning in flame'. Cf. footnote 22 below.

[19] *Op. cit.*, 139.

[20] *Loc. cit.*

[21] *Loc. cit.*, footnote.

[22] This time the need of modifiers is mentioned (cf. footnote 18 above).

[23] *Op. cit.*, 139f.

[24] On the applications of transformation analysis to diachronic syntax, see 0.311 and fn. 44 below.

present paper suggests an entirely different approach to this same problem of classification, an approach based nearly as exclusively on form as the traditional approach was based on meaning.[25] The technique offered here consists fundamentally of examining each unit to be classified from two points of view, first that of what it is (the traditional morphological classification, valid as far as it goes), and then that of what it can become, of what specific changes can and cannot be wrought upon it. These changes will be called transformations, conforming to the terminology used by Chomsky and Harris,[26] but it is to be noted that this paper attempts to use these transformations for one restricted purpose only, namely to classify otherwise identical phrases. It is not offered as one ready-made section of a complete transformation syntax of Russian. The working out of such a syntax is a different and more complicated operation than that attempted here, although it is hoped, of course, that the problems and solutions encountered in the present paper may contribute to the eventual development of such a full-scale syntactic description.

0.21. Morphological Classification. Transformation analysis proceeds in two steps: (1) a preliminary morphological classification of phrase types; (2) a transformation classification of subtypes within each morphologically defined phrase type. The preliminary morphological classification is based on a number of phrases occurring in a given corpus.[27] It presupposes that (1) we know all the major word classes of the language in question and (2) we can recognize the class membership of all words occurring in our given phrases.[28] Each phrase is described as a string of class members, each of which expresses certain grammatical

[25] For further comparison of these two approaches, see 7.0.

[26] See footnote 12. The present writer made a few brief steps toward the use of transformations in syntactic analysis in his unpublished dissertation, "A Contribution to the Study of the Syntactic Binary Combination in Contemporary Standard Russian," Harvard University, 1956.

[27] The units analyzed here were culled from some 16,000 syntactic combinations excerpted from Soviet literature by collaborators on the Russian Language Research Project directed by Professor Roman Jakobson of Harvard University and the Massachussetts Institute of Technology and supported by the Rockefeller Foundation, whose help is gratefully acknowledged. This material was supplemented by instrumental constructions taken from the works cited in footnote 3 above and from D. N. Usakov, ed., *Tolkovyj slovar' russkogo jazyka*, I-IV, Moskva, 1935-40.

[28] In a highly inflected language like Russian, class membership can in most cases be determined by purely morphological criteria; none of the cases where such determination is impossible is pertinent to the present investigation.

categories (knowledge of which is also presupposed), e.g. the phrase "The dog was chewing the bone" might be described as $NP_{sing.}$ animate+$V_{sing.}$ past progressive+$NP_{sing.}$ inanim.

0.211. REDUCTION. The phrases which actually occur in any given corpus contain many items (groups of words, or individual morphemes) which are superfluous to the particular constructions being investigated. To avoid cluttering the preliminary morphological classification with irrelevant details, all actually occurring phrases are first reduced to the structural essentials necessary for further analysis. There are two kinds of reduction. First, all modifiers are eliminated from endocentric constructions,[29] excepting only those very units which we are interested in classifying. For example, should we be interested in phrases containing "by+NP" in English ("by John," "by moonlight"), which we find to occur in the sentences "The biggest fish of the season was caught by old John Davis last night" and "All the cargo was unloaded from the ships by moonlight because of the impending strike," we reduce these sentences to "The fish was caught by John" and "The cargo was unloaded by moonlight." Similarly, the Russian phrase *Bol'šaja gostinnaja komnata v dome Ivanovyx uže napolnjalas' tolpoj ženščin i detej* 'The big living room in the Ivanovs' house was already being filled by a crowd of women and children' can, if we are interested in the instrumental substantive *tolpoj* 'by a crowd', be reduced to *komnata napolnjalas' tolpoj* 'the room was being filled by the crowd' without losing anything essential to the construction we are trying to analyze.[30]

The second step of reduction consists of eliminating from the description all those grammatical categories which can be shown to be irrelevant to the transformations to be effected. This second elimination, although in practice based on intuition in many cases, can always be justified by a rigorous procedure which puts the given phrase through all possible transformations and only then eliminates as irrelevant those categories which remain constant throughout all transformations and which can be varied freely without either increasing or restricting the number of possible transformations. We will find, for example, that the categories

[29] See Leonard Bloomfield, *Language*, New York, 1933, p. 194f.

[30] Within the framework of transformation syntax, this process of reduction can itself be considered a series of transformations of the forms F^1, F^2 etc. to zero (T: $F^1 \rightarrow \emptyset$ etc.); the opposite process can then be called expansion and considered a series of transformations T: $\emptyset \rightarrow F^1$, T: $\emptyset \rightarrow F^2$, etc. Cf. 0.221.

"tense" and "number" are irrelevant to the active—passive transformation in English, and if dissatisfied with our intuitive perception of this fact, we can prove it by letting F=an active sentence and F′ =the passive transform thereof and noting that the relation between F and F′ is identical in all cases of F → F′, regardless of which morphemes of tense or number happen to occur, e.g.

John saw the boy → The boy was seen by John
John will see the boy → The boy will be seen by John
John saw the boys → The boys were seen by John

etc. Similarly, the relation between F and F′ remains constant in the Russian examples:

tolpa napolnjael komnatu 'the crowd fills the room'
→ *komnata napolnjaetsja tolpoj* 'the room is filled by the crowd'

tolpa napolnjael komnaty 'the crowd fills the rooms'
→ *komnaty napolnjajutsja tolpoj* 'the rooms are filled by the crowd'

tolpa napolnjala komnatu 'the crowd was filling the room'
→ *komnata napolnjalas' tolpoj* 'the room was being filled by the crowd'

etc., which entitles us to eliminate tense and number from consideration as far as this particular transformation is concerned.

Once the phrase has been reduced to its structural essentials, it can be represented by a string of symbols expressing class membership and relevant grammatical categories,[31] e.g. in English we will write:

John caught the fish
→ The fish was caught by John as S^1 V S^2 → S^2 isVen byS^1

and in Russian:

[31] The following symbols are used in this paper: S_n, S_g, S_a, S_i = substantive in the nominative, genitive, accusative and instrumental cases respectively, pS = preposition+governed substantive; A = adjective (same case subscripts as for substantives), Aø = zero (adverbial) form of adjective; V = verb, V_s = so-called "reflexive" verb in *sja* or *-s'*, V_p = perfective aspect verb, Vø = "impersonal" verb in neuter past or 3d sing. nonpast, V_- = "anonymous" or subjectless verb in plural past or 3d plural nonpast, # = zero nonpast form of *byl'* 'be' or any zero form, *byl-* = any past form of *byl'*; ø = absence of a form (opposed to its presence in a particular construction); F = any form (single word, phrase, etc.), F′ = a transformational variation of F; F^1, F^2, F^3 = instances of F; NP = Noun phrase; superscript numerals = consecutive occurrences of members of a single class, e.g. S^1, S^2 = consecutive substantives;

tolpa napolnjael komnatu
→ *komnata napolnjaetsja tolpoj* as S^1_n V S^2_a → S^2_n V_s S^1_i

All phrases which have been reduced and symbolized can then be classified into groups of like morphological form. This preliminary classification obviously throws together phrases of different structure and meaning, e.g. the class S^1 isVen byS^2 includes "Mary was kissed by moonlight" as well as "Mary was kissed by John". It is the job of transformation analysis to describe the formal distinctions between such morphologically identical phrases.

0.22. Transformation Operations. All reduced phrases are then tested to see in which ways they can and cannot be transformed, and each class of morphologically identical phrases is divided into subclasses according to the various sets of transformation which obtain for the phrases of this class.[32]

0.221. Types of Transformation. There are a number of different types of transformation, not all of which are equally pertinent to the present investigation. Most important for our purposes are what may be called intraclass transformations, effected within a morphologically determined form class, e. g. substitution of a group "preposition+substantive" for a substantive in English or substitution of one case for another in Russian, e.g. T: S_n → S_a *komnata* → *komnatu*, or the change of active to passive verb forms in either language, e.g. T: V → isVen "bit" → "was bitten" or T: V → V_s *napolnjala* → *napolnjalas'*. Addition and elimination of forms are most conveniently represented as transformations from and to zero units (T: ø → F, T: F → ø), since in such cases the presence of a form in one of two transforms is correlated with its absence in the other. Other types of transformation are of lesser importance for this paper.[33] Individual transformations will be described as they occur.

→ = is transformed to, ← = is transformed from; + and — = dividers between members of a string (graphic device; no grammatical significance); * = impossible form; WO = word order. Other symbols will be explained as they occur in the text.

[32] Cf. 7.1 for some of the implications of these operations.

[33] We shall mention interclass transformations, which shift a word from one form class to another (*kurit'* 'to smoke' → *kuren'e* 'smoking', *zelenyj* 'green' → *zelenet'* 'to show green', etc.) and are of great importance for problems of complete syntactic description (v. J. Kuryłowicz, "Dérivation lexicale et dérivation syntaxique (contri-

Transformations can be described either individually or, when they imply each other, as complete sets, or phrase transformations. The active—passive transformation in English, for example, consists of three individual transformations T: V → isVen, T: S^1 → byS^1, and the word-order transformation (difficult to symbolize) which has the effect of changing the places of S^1 and S^2; these three transformations imply each other and can be written as a single phrase transformation:

S^1 V S^2 The dog bit the man
→ S^2 isVen byS^1 → The man was bitten by the dog.[34]

0.222. TESTING PROCEDURES. The method by which it is determined which transformations can and which cannot be applied to a given phrase can be formulated in rigidly systematic terms: given a phrase consisting of the words X+Y+Z, we apply each possible intraclass transformation to X and note what if any transformations must be applied to Y and Z if the result is to be a grammatical phrase; the same procedure is then repeated with Y and Z. For example, given the phrase "The dog bit the man", we can if necessary go through the procedure of applying, e.g., T: S^1 → byS^1 ("the dog" → "by the dog"), and note that if we also apply T: V → isVen and the word-order reversal of S^1 and S^2, we obtain the grammatical phrase "The man was bitten by the dog", whereas transformations producing "from the dog", "with the dog", etc. cannot result in grammatical phrases no matter what is done to V and S^2. Similarly, in Russian, given the phrase *tolpa napolnjala komnatu* 'the crowd was filling the room', we can apply T: $S^1{}_n \rightarrow S^1{}_i$ and obtain the grammatical phrase *komnata napolnjalas' tolpoj*, provided we also apply T: $S^2{}_a \rightarrow S^1{}_n$ and the same word-order reversal as in the English example above.[35] In practice, such rather tortuous procedures are often developed to explain the intuitive jump from one grammat-

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bution à la théorie des parties du discours)," *Bulletin de la Société de Linguistique de Paris*, XXXVII, 2, [1952] pp. 79-92), but which have only occasional significance for this paper (cf. 2.1211, 2.1222); word-order transformations are of more importance in English than in Russian, where their use is primarily stylistic (*ja znal èto* 'I knew that' → *èto ja znal* 'I knew *that*', etc.). It should perhaps be noted that all transformation operations in Russian presuppose a set of morphophonemic rules which (by making verb agree with subject, etc.) will produce grammatical phrases from the transforms produced by the transformation rules.

[34] Tense is irrelevant in this transformation; cf. 0.211.

[35] Such procedures are considerably less artificial in Russian, the elaborate case system of which makes possible many intraclass transformations for each substantive.

ical phrase to another; i.e., one usually proceeds by whole phrase transformations, not by accumulations of individual transformations.

As the analysis of individual phrases continues, these are found to undergo partially the same, partially different transformations. In English, for example, we find many phrases which can undergo both the active—passive voice transformation and a transformation from non-progressive to progressive aspect, such as the phrase

The dog bit the man

which can → The man was bitten by the dog (T_{pass})
and also → The dog was biting the man (T_{prog})
and even both → The man was being bitten by the dog ($T_{pass}+T_{prog}$)

although the apparently identical phrase,

The dog chewed the bone

can undergo T_{pass} only if it also undergoes T_{prog}, namely,
it cannot → *The bone was chewed by the dog[36] (T_{pass})
but it can → The dog was chewing the bone (T_{prog})
and it can also → The bone was being chewed by the dog ($T_{pass}+T_{prog}$).[37]

0.3. Form and Meaning. Two phrases which are transforms of each other are correlated in meaning as well as in form. This is not to say that their meanings are identical (on the contrary, one assumes *a priori* that each difference in form corresponds to a difference in meaning), but rather that there is a constant difference between the meanings of individual units of correlated transform pairs, i.e. that in any series of transformations $F^1 \rightarrow F'^1, F^2 \rightarrow F'^2, \ldots F^n \rightarrow F'^n$, the referential meaning of F is related to (differs from) that of F′ in exactly the same way in each of

[36] Barring some particular environment, this will usually be interpreted as a resultative (where "was chewed" could be substituted by "had been chewed," which could not be done in the case of "The man was bitten by the dog"); this is a case of noncorrelation of meaning as explained in 0.3.

[37] Such differences in the ability to undergo transformations are one of the most productive, if not the most productive, means of distinguishing between synonyms (if indeed such exist). For example, the verbs "like" and "enjoy" are probably considered synonymous by most English speakers. However, transformation analysis reveals a marked difference: whereas the phrase "The critic enjoyed the play" can undergo passive and/or progressive transformations (→ "The play was enjoyed by the critic", → "The critic was enjoying the play", → "The play was being enjoyed by the critic"), the superficially synonymous phrase "The critic liked the play" can undergo only T_{pass}, not T_{prog} (→ "The play was liked by the critic", but neither → *"The critic was liking the play" nor → *"The play was being liked by the critic").

the series of pairs. Should this regular correlation of meaning fail to obtain for some pair $F^x \rightarrow F'^x$ formally belonging to this series, this fact is to be considered a danger signal indicating that the formal possibility of T: $F^x \rightarrow F'^x$ may in reality be a superficial or non-productive feature concealing (or, better, not uncovering) some more essential transformation feature which makes it impossible to consider $F^x \rightarrow F'^x$ a true instance of $F \rightarrow F'$.[38] For example, in the English progressive aspect transformation S^1 V $S^2 \rightarrow S^1$ isVing S^2, the regular meaning correlation obtaining in all cases of $F \rightarrow F'$ in the examples "John eats the apple" → "John is eating the apple", "My wife cooks supper" → "My wife is cooking supper", etc., suddenly fails to obtain in the instance "John sees the boy" → "John is seeing the boy"; this is our clue to seek other transformation features distinguishing "John sees the boy" from "John eats the apple", "My wife cooks supper", etc.[39] Similarly, we find that in one type of passive—active transformation in Russian, namely S^1_n V_s $S^2_i \rightarrow S^2_n$ V S^1_a, we find that the meaning of F differs from that of F′ in exactly the same way in each of the instances of $F \rightarrow F'$: *komnata napolnjalas' tolpoj* 'the room was filled by the crowd' → *tolpa napolnjala komnatu* 'the crowd filled the room', *zala osveščaetsja fonarikami* 'the room is lighted by lanterns' → *fonariki osveščajut zalu*, *simfonija ispolnjaetsja orkestrom* 'the symphony is played by the orchestra' → *orkestr ispolnjaet simfoniju*, but in the formally identical instance *Ivan vernulsja starikom* 'John came back an old man' → *starik vernul Ivana* 'the old man brought John back' the expected correlation does not obtain, which is a signal that we must look elsewhere for differences between *Ivan vernulsja starikom* and the other S^1_n V_s S^2_i phrases just cited.[40]

[38] One cannot of course have recourse to meaning alone, but a sharp difference in meaning may well be the clue to an equally sharp, if not equally obvious, difference in form. A good many seeming differences in meaning unaccompanied by formal distinctions may be due primarily to our as yet rather naïve conception of linguistic form.

[39] One finds such features, e.g., in the fact that "John eats the apple noisily" can → "John is eating the apple noisily", but "John sees the boy clearly" cannot → *"John is seeing the boy clearly"; such features will probably eventually separate out and formally characterize all verbs of perception.

[40] The principal difference is that *Ivan vernulsja starikom* is a case of simultaneous double predication resulting from the combinatory transformation of two kernel sentences *Ivan vernulsja* 'John came back' and *Ivan starik* 'John is an old man', whereas the other phrases are simple passive transforms (e.g., one cannot derive *zala osveščaetsja fonarikami* from a combination of *zala osveščaetsja* and *zala fonariki*).

0.31. DIRECTIONAL TRANSFORMATIONS. The problem of meaning correlations discussed in 0.3 is closely connected with that of the direction in which transformations proceed. It has been suggested that the rather awkward requirement that transforms be correlated in meaning as well as in form could be eliminated by stating that (1) transformations are unidirectional and (2) instrumental constructions are not basic but are derived from other kernels; this would permit the statement that *starik vernul Ivana* 'the old man brought John back' is a kernel and, because of the perfectivity—animation rule described in 1.112 below, one cannot derive *Ivan vernulsja starikom* (regardless of its meaning) therefrom.[41] Now, while this viewpoint provides a welcome rule eliminating all formal connection between *Ivan vernulsja starikom* 'John came back an old man' and *starik vernul Ivana* 'the old man brought John back', it raises some broader theoretical problems which ought not to pass unnoticed. For one thing, while there is very probably a hierarchy of phrase structures in all languages, and while the relation between certain phrase types may be most economically described as sets of transformations proceeding in a certain direction (this seems to be the case with active—passive constructions in English as well as Russian[42]), it is equally true that given the correlated transforms F and F′ (i.e., given the existence of the phrase types F and F′ and a statable procedure for deriving one from the other), there is no reason to assume *a priori* that the derivation proceeds in one direction rather than the other (the formal description is just as easy in terms of F′ → F as in terms of F → F′). There is, as a matter of fact, no very good reason for assuming that the relation between correlated transforms must be that of unidirectional derivation (i.e., for positing automatic hierarchy between these phrase types). There are compelling historic reasons for asserting that this cannot always be the case.

[41] This argument goes on to say that had the kernel verb been imperfective *(vozvraščal)*, the transformation would have been possible. This is not quite true, however, since the phrase *Ivan vozvraščalsja starikom* is, if not impossible, at least restricted to a few specific contexts, regardless of its meaning ('John came back an old man' or 'John was brought back by the old man'), i.e. regardless of its derivational history (← *Ivan vozvraščalsja*+*Ivan starik* or ← *starik vozvraščal Ivana*). A more accurate statement might be that S^1_n V $S^2_a \rightarrow S^2_n$ V_e S^1_i is impossible wherever S^2 is animate; with inanimate S^2 and animate S^1, e.g. *buxgalter sostavljaet sčet* 'the bookkeeper is making up the account', the same transformation is possible provided only V is not perfective; cf. 1.112. For other restrictions on this type of construction, see V. V. Vinogradov, *Russkij jazyk*, 633.

[42] For the arguments concerning English. see Noam Chomsky, *Syntactic Structures*, 80.

0.311. DIACHRONIC SYNTAX. If we look briefly at syntax from the diachronic rather than from the synchronic point of view, we see that (1) systems of correlated transforms provide the most convenient framework for discussing the historical evolution of syntactic forms, and (2) a description which considers all transformations to be unidirectional presupposes the demonstrable untruth that syntactic patterns are static. Assuming that a hierarchic distinction between kernel and derivative may but need not obtain between correlated transforms, and once it has been established that F and F′ are correlated transforms, there are three possible transformational relations between them: (1) neither F nor F′ can be shown to be the kernel from which the other is derived, i.e., F and F′ are simply coexisting and interchangeable phrase types, not necessarily identical in meaning (type F $\longleftrightarrow$ F′); (2) one type can be proved derivative from the other, namely either (2a) F is a kernel and F′ a derivative (F $\rightarrow$ F′) or (2b) F′ is a kernel and F derived therefrom (F $\leftarrow$ F′). The indisputable fact that with the passage of time constructions of one type succeed constructions of another type leads us to posit a succession of five stages (which, in actual historical fact, would flow imperceptibly each into the next):

(1) F — F exists alone (the type F′ has not yet been used)

(2) F ($\rightarrow$ F′) — F is the kernel, but can $\rightarrow$ F′ (F is more common, but the type F′ is growing)

(3) F $\longleftrightarrow$ F′ — F and F′ are fully interchangeable

(4) (F $\leftarrow$) F′ — F′ is the kernel, but can $\rightarrow$ F (F is felt as archaic, but still used occasionally)

(5) F′ — F′ exists alone (F is found in older texts only).[43]

[43] As an example of this process, consider the replacement of the "instrumental of cause", e.g. *sdelal' ošibkoju* 'do by mistake', by various analytic constructions of the type pS, e.g. *po ošibke* 'by mistake', *iz-za gluposti* 'because of stupidity', *ot ustalosti* 'from tiredness'. Taking one of the latter, we let F = the phrase type V S and F′ = the phrase type V iz-za S_g, and we can fix the broad outlines of the historical development as follows:

(1)	F	Old Russian, through 16th century
(2)	F ($\rightarrow$ F′)	17th century
(3)	F $\longleftrightarrow$ F′	18th century
(4)	(F $\leftarrow$) F′	19th century
(5)	F′	Modern Russian.

For details of dates and examples, see T. P. Lomtev, *Očerki po istoričeskomu sintaksisu russkogo jazyka*, Moskva, 1956, pp. 247f., 386f.

Conversely, any synchronic slice should try to fix the relation between two correlated transforms as one of the three stages $F \rightarrow F'$, $F \leftrightarrow F'$, or $F \leftarrow F'$; failure to do so is to further the outdated Saussurian equation of synchronic with static. The assumption that all derivational relations are of the type $F \rightarrow F'$ is therefore seen to be incompatible with historical fact and consequently unacceptable even in synchronic analysis.[44]

0.4. PLAN OF ANALYSIS. In what follows, the analytic technique outlined above has been applied to Russian constructions in which instrumental substantives modify finite verbal forms. Considerations of space require a degree of symbolization which is, at times, unfortunately high; often, only one example of the more common types of subclass is given.

With insignificant exceptions, there are after reduction (cf. 0.211) six morphologically distinct types of phrase in which instrumental substantives modify verbs:

1. $S^1_n\ V_s\ S^2_i$: *komnata napolnjalas' tolpoj* 'the room was being filled by the crowd', *luga zalilis' vodoj* 'the meadows were flooded with water', *sčet sostavljaetsja buxgalterom* 'the account is drawn up by the bookkeeper', *učreždenie rukovoditsja rabotnikom* 'the establishment is managed by a worker', *student udarilsja nožom* 'the student wounded himself with a knife', *Ivan vernulsja starikom* 'John came back an old man', *barži tjanulis' rjadami* 'the barges moved along in rows', *Boris vernulsja večerom* 'Boris came back in the evening'.[45]

2. $S^1_n\ V\ S^2_i$: *rabotnik rukovodit učreždeniem* 'the worker manages

[44] The second half of the suggested solution to the problem posed by *Ivan vernulsja starikom* → *starik vernul Ivana* is also open to criticism. There is no good reason to assume that instrumental constructions are *per se* secondary forms, derived from kernels of different structure. This assumption, which may be based on a faulty equation of the Russian instrumental substantive S_i with the English "passive actor" byS (cf. footnote 1), fails to take into account the many instrumental constructions which cannot, to my knowledge, be derived from other phrase types, e.g. *kapitan komanduet batal'onom* 'the captain commands the batallion', *on povel brovjami* 'he raised his eyebrows', *student udaril professora nožom* 'the sutdent struck the professor with the knife'. Eventually it will probably prove to be the case that, within a morphologically defined phrase type, some of the units will be kernels and others secondary transforms derived from other phrase types. The present paper cannot go into this problem of categorical hierarchies in any detail.

[45] All examples given for this and the following morphologically described classes are distinguished from each other by transformation features described in the individual sections to follow.

the establishment', *kapitan komanduet batal'onom* 'the captain commands the batallion', *Ivan pokačal golovoj* 'John nodded his head', *on povel brovjami* 'he raised his eyebrows', *Ivan priexal starikom* 'John arrived an old man', *ona vyla šakalom* 'she howled like a jackal', *oni šli verenicej* 'they went in a row', *Boris čitaet večerom* 'Boris reads in the evening', *oni šli lesom* 'they were walking through the forest', *on govoril šopotom* 'he speaks in a whisper'.

3. S^1_n V A_i S^2_i : *on govoril nizkim tonom* 'he spoke in a low voice', *on kričal gromkim golosom* 'he shouted in a loud voice', *on smotrel ostorožnymi glazami* 'he looked with cautious eyes'.

4. S^1_n V S^2_a S^3_i: *oni vybrali ego prezidentom* 'they elected him president', *ja znal ego studentom* 'I knew him as a student', *ja sčitaju ego durakom* 'I consider him a fool', *on zakryl dver' rukoj* 'he closed the door with his hand', *on udivil nas otvetom* 'he astonished us by his answer', *rabočie pokryli ulicu asfal'tom* 'the workers covered the street with asphalt', *tetja nadelila menja nasledstvom* 'my aunt left me an inheritance'.

5. Vø S_i : *zateklo krov'ju* 'blood began to flow', *popaxivaet dymom* 'it smells rather of smoke'.

6. S^1_a Vø S^2_i : *šljapu uneslo vetrom* 'the hat was carried off by the wind', *otca pereexalo avtomobilem* 'father was run over by a car', *luga zalilo vodoj* 'the meadows were flooded with water'.

Sections 1—6 below will discuss these six phrase types in some detail and point out many of the transformationally determined varieties within each type.

1. PHRASE TYPE S^1_n V_s S^2_i.

1.1. Units of type S^1_n V_s S^2_i can be classified as containing subjective, semi-subjective, and non-subjective instrumental modifiers. The subjectivity or non-subjectivity of S^2_i is formally expressed in the possibility or impossibility of the transformation T: → S^2_n V S^1_a or (rarely) → S^2_n V S^1_i. Subjective and semi-subjective units appear to be derivative from correlated transforms.

1.11. Subjective instrumental modifiers occur in units where the transformation T: → S^2_n V $S^1_{a/i}$ is possible, e.g. *komnata napolnjalas' tolpoj* 'the room was being filled up by the crowd' → *tolpa napolnjala komnatu*, *luga zalivalis' vodoj* 'the meadows were flooded with water' → *voda zalivala luga*, *sčet sostavljaetsja buxgalterom* 'the account is made up by the bookkeeper' →

buxgalter sostavljaet sčet, učreždenie rukovoditsja rabotnikom 'the establishment is managed by a worker' → *rabotnik rukovodit učreždeniem.* These units can be divided into two groups, according to whether or not the verb can occur in the perfective aspect (formally, whether or not T: $V_s \rightarrow V_{sp}$ is possible).

1.111. Units in which both S^1 and S^2 are inanimate substantives are not restricted as to aspect, e.g. *komnata napolnjalas' tolpoj* 'the room was being filled up by the crowd' / *komnata napolnilas' tolpoj* '... was filled...', *luga zalivalis' / zalilis' vodoj* 'the meadows were being flooded / were flooded with water', nor are their S^2_n V S^1_a transforms, e.g. *tolpa napolnjala / napolnila komnatu, voda zalivala / zalila luga.*

1.112. Units in which S^1 is an inanimate and S^2 an animate substantive can occur only in the imperfective aspect (i.e., T: $V_s \rightarrow V_{sp}$ is impossible), e.g. *sčet sostavljaetsja buxgalterom* 'the account is made up by the bookkeeper' cannot → **sčet sostavitsja buxgalterom*, similarly in the past *sčet sostavljalsja buxgalterom* cannot → **sčet sostavilsja buxgalterom.* This restriction of aspect does not apply to the S^2_n V S^1_a transforms of these units, e.g. *buxgalter sostavljaet / sostavit sčet* 'the bookkeeper makes / will make up the account', *buxgalter sostavljal / sostavil sčet.*[46] If we accept the statement, "Of two correlated transforms, the one having the lesser number of transformation restrictions is to be considered basic, and the other a derivative thereof," we will then consider the present (1.112) S^1_n V_s S^2_i units to be derived from their correlated S^2_n V S^1_a transforms.

1.113. In one infrequent type of subjective instrumental unit, the original T: $S^2_i \rightarrow S^2_n$ entails not $S^1_n \rightarrow S^1_a$ but $S^1_n \rightarrow S^1_i$, producing the transform S^2_n V S^1_i, e.g. *učreždenie rukovoditsja rabotnikom* 'the establishment is managed by a worker' → *rabotnik rukovodit učreždeniem.* Cf. 2.1122.

1.114. One type of S^1_n V_s S^2_i unit is characterized by the possibility of a further transformation T: → S^1_a Vø S^2_i, e.g. *luga zalilis' vodoj* 'the meadows were flooded with water' → *luga zalilo vodoj.* Cf. 6.12.

1.12. Semi-subjective instrumental modifiers occur in units

[46] It was Roman Jakobson who first called this fact to the author's attention; cf. also V. V. Vinogradov, *Russkij jazyk*, 633.

where S^1 is an animate and S^2 an inanimate substantive. The subjective transformation T: → S^2_n V S^1_a is usually possible but awkward (i.e., less grammatical than in the case of the subjective units in 1.11 above), e.g. *student udarilsja nožom* 'the student struck himself with the knife' → *nož udaril studenta.* This semi-subjective status of S^2, however, is much less important than the fact that this type of unit can be transformed by T: → S^1_n V S^3_a S^2_i, e.g. → *student udaril professora nožom* 'the student struck the professor with the knife'; this transformation is impossible for both subjective and non-subjective units. The S^1_n V_s S^2_i unit is probably to be considered a derivative of the S^1_n V S^3_a S^2_i transform; cf. 4.

1.13. In units with non-subjective instrumental modifiers the transformation T: → S^2_n V $S^1_{a/i}$ is either impossible or involves such a shift in referential meaning (cf. 0.3 above) that the two units S^1_n V_s S^2_i and S^2_n V S^1_a cannot be considered correlated transforms of each other, e.g. *Ivan vernulsja starikom* 'John came back an old man' → **starik vernul Ivana* 'the old man brought John back', *barži tjanulis' rjadami* 'the barges moved in rows' → **rjady tjanuli barži* 'the rows (e.g. of men) pulled the barges'. These non-subjective units can be divided into two groups, containing predicative and non-predicative instrumental modifiers, according to whether or not the verb can be transformed to a form of the verb 'to be' (formally, whether T: V_s → byl- is possible).

1.131. In units with predicative instrumental modifiers the transformation T: → S^1_n byl- S^2_i is possible, e.g. *Ivan vernulsja starikom* 'John came back an old man' → *Ivan byl starikom* 'John was an old man'. This predicative instrumental unit can be derived from the combination of two simpler units *Ivan vernulsja* 'John came back' and *Ivan byl starikom* 'John was an old man' either directly or through some intermediate step such as *kogda Ivan vernulsja, on byl starikom* 'when John came back, he was an old man'. The non-subjectivity of S^2_i in these units finds further formal expression in the fact that it can usually be omitted (T: S^2 → ø), e.g. → *Ivan vernulsja;* cf. **učreždenie rukovodilsja* 'the establishment is managed', etc. Cf. 2.1211.

1.132. In units with non-predicative instrumental modifiers the transformation T: → S^1_n byl- S^2_i is impossible, e.g. *barži tjanulis' rjadami* 'the barges moved in rows' → **barži byli rjadami* 'the barges were rows', but one or more of a number of prepositional transformations T: → S^1_n V_s pS^2 is possible, e.g. → *barži tjanulis'*

v rjadax 'the barges moved in rows'. S^2_i is always either a temporal or a spatial modifier; the individual words occurring as S^2_i can be listed as temporal or spatial according to other formal criteria (e.g. whether or not the word can be used in the accusative to modify verbs in *-sja*, etc.).

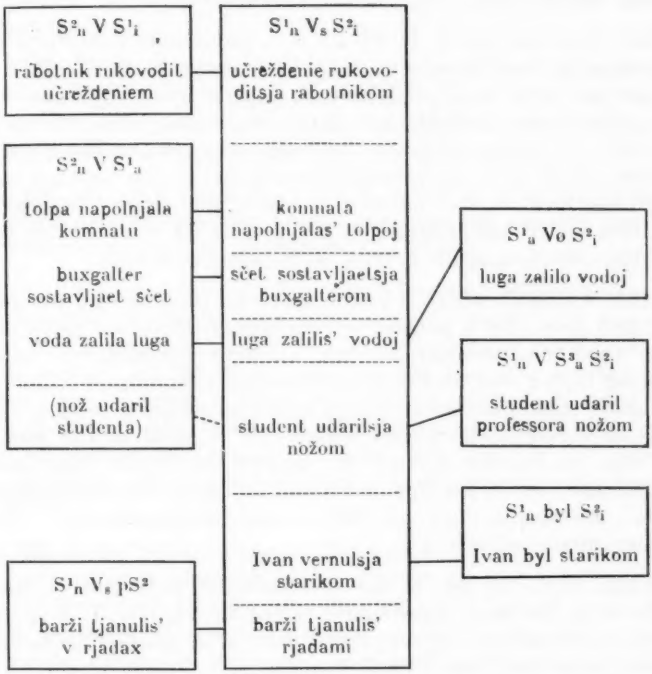
1.2. The possibility or impossibility of a particular units undergoing each of the set of possible transformations can be represented in tabular form as follows:

TABLE 1A : TRANSFORM FEATURES OF S^1_n V_s S^2_i UNITS

T : →	S^s_n V S^1_a	S^s_n V St_i	V_{sp}	S^1 V∅ S^s_i	S^1_n V S^s_a, S^s_i	S^1_n byl- S^s_i	S^1_n V_{sp} S^s	
	+	—	+	—	—	—	—	komnata napolnjalas' tolpoj
	+	—	+	+	—	—	—	luga zalivalis' vodoj
	+	—	—	—	—	—	—	sčet sostavljaetsja buxgalterom
	- -	+	—	—	—	—	—	učreždenie rukovoditsja rabotnikom
	(+)	—	+	—	+	—	—	student udarilsja nožom
	(—)	—	+	—	—	+	—	Ivan vernulsja starikom
	(—)	—	—	—	—	—	+	barži tjanulis' rjadami

1.3. The network of correlated transforms in which units of type S^1_n V_s S^2_i participate can be represented schematically as follows:

TABLE 1B: TRANSFORM NETWORK OF S^1_n V S^2_i UNITS



2. PHRASE TYPE S^1_n V S^2_i.

2.1. Units of type S^1_n V S^2_i can be classified as containing central or marginal instrumental modifiers,[47] according to whether the instrumental substantive cannot or can be omitted from the given unit (i.e., whether T: $S^2_i \rightarrow \emptyset$ is impossible or possible), e.g. on the one hand *on čital večerom* 'he read in the evening' → *on čital*, *ona vyla šakalom* 'she howled like a jackal' → *ona vyla*, but on the other *rabotnik rukovodit učreždeniem* 'the worker manages the establishment' → **rabotnik rukovodit*, *on pokačal golovoj* 'he nod-

[47] In another sense all instrumentals can be considered marginal; see Roman Jakobson, "Beitrag zur allgemeinen Kasuslehre: Gesamtbedeutungen der russischen Kasus", *Travaux du Cercle Linguistique de Prague*, VI (1936), pp. 240-288.

ded his head' → **on pokačal.* Units with central instrumental modifiers fall into two, and units with marginal modifiers into several, sub-groups.

2.11. Units in which T: $S^2_i \rightarrow \emptyset$ is impossible contain central instrumental modifiers, e.g. *rabotnik rukovodit učreždeniem* 'the worker manages the establishment', *kapitan upravljaet batal'onom*, 'the captain commands the batallion', *on pokačal golovoj* 'he nodded his head', *on podergival nosom* 'his nose twitched'. There are two obvious sub-groups, the principal formal distinction between which lies in the high vs. low number of restrictions upon the adjective modifiers which can be added to S^2_i (i.e., whether for certain types of A the transformation T: $\emptyset \rightarrow A_i$ is possible or not).

2.111. Units in which S^2_i can rarely be modified by an adjective, and never by a possessive pronominal adjective referring to other than S^1_n, contain as S^1_n animate substantives usually referring to persons, as V verbs expressing a motion of some kind, and as S^2_i inanimate substantives referring either to a part of the body of S^1_n or to an object which can be held in the hand of S^1_n, e.g. *on pokačal golovoj* 'he nodded his head', *ona ševelila gubami* 'she moved her lips', *on brosal kamnjami* 'he was throwing stones', *oni maxali platkami* 'they waved their kerchiefs'. There are two minor sub-groups.

2.1111. Units in which the instrumental substantive can be replaced by the same substantive in the accusative (T: $S^2_i \rightarrow S^2_a$) contain such units as e.g. *on pokačal golovoj* 'he nodded his head' → *on pokačal golovu*, *ona brosala kamnjami* 'she threw stones' → *ona brosala kamni*, *on razvel rukami* 'he spread his hands' → *on razvel ruki*. The S^1_n V S^2_a transforms are not limited in T: $\emptyset \rightarrow A_i$ transformations, e.g. *on razvel ruki* 'he spread (his) hands' → *on razvel ix ruki* 'he spread their hands' (cf. *on razvel rukami* → **on razvel ix rukami*).

2.112. Units in which T: $S^2_i \rightarrow S^2_a$ is impossible do not differ noticeably in meaning from 2.1111 units, e.g. *on podergival nosom* 'his nose twitched', *on povel brovjami* 'he raised his brows'.

2.1121. Units in which S^2_i can with very few restrictions be modified by adjectives (i.e., where T: $\emptyset \rightarrow A_i$ is possible for many types of A) usually contain as S^1_n an animate substantive referring to a person, as V a verb with the general meaning of directing, exercising influence over, and as S^2_i an inanimate substantive most

frequently referring to a collectivity, e.g. *rabotnik rukovodit učreždeniem* 'the worker manages the establishment', *kapitan upravljaet batal'onom* 'the captain commands the batallion'. There are two sub-groups.

2.1121. In most cases no active → passive transformation T: → S^2_n V_s S^1_i is possible (since there is no V_s form of V), e.g. *kapitan komanduet batal'onom* 'the captain commands the batallion' → **batal'on komanduetsja kapitanom.*

2.1122. In a few cases T: → S^2_n V S^1_i is possible, e.g. *rabotnik rukovodit učreždeniem* 'the worker manages the establishment' → *učreždenie rukovoditjsa rabotnikom.* Cf. 1.113.

2.12. Units in which T: $S^2_i \rightarrow$ ø is possible contain marginal instrumental modifiers (this label actually being only a restatement of the possibility of T: $S^2_i \rightarrow$ ø), e.g. *Ivan priexal starikom* 'John arrived an old man', *ona vyla šakalom* 'she howled like a jackal', *oni šli verenicej* 'they went in a row', *oni šli lesom* 'they walked through the forest', *Boris čital večerom* 'Boris read in the evening', *on govoril šopotom* 'he spoke in a whisper'. There are two principal and several smaller groups of unit with marginal modifiers.

2.121. Units in which the transformation T: → S^2_n V is possible, e.g. *Ivan priexal starikom* 'John arrived an old man' → *starik priexal, ona vyla šakalom* 'she howled like a jackal' → *šakal vyl, oni šli verenicej* 'they went in a row' → *verenica šla* can be termed analogous units (in the sense that each contains an analogy), which express a temporary identity or similitude between S^1 and S^2. Analogous units are subdivided into predicative and non-predicative units, and the latter further divided into comparative and metamorphic.

2.1211. Units in which the transformation T: → S^1_n byl- S^1_i is possible contain predicative instrumental modifiers, e.g. *Ivan priexal starikom* 'John arrived an old man' → *Ivan byl starikom* 'John was an old man'. The label 'predicative' is itself obtained from a form of this transform, e.g. T: → kogda S^1_n V, P_{sln} byl- $S^2_{i\cdot n}$ (where P_{sln} = a pronominal substantive referring to S^1_n), e.g. *kogda Ivan priexal, on byl starikom* 'when John came, he was an old man'. Predicative units can always be derived from a combination of two simpler units with common S^1_n, e.g. (*Ivan priezžaet* 'John comes' + *Ivan starik* 'John is an old man') T_{past} =

(*Ivan priexal*+*Ivan byl starik*[*om*]) = *Ivan priexal starikom*, cf. the similar derivations *Ivan zdes'* 'John is here'+*Ivan sud'ja* 'John is a judge' = *Ivan zdes' sud'ej* 'John is here as a judge' and perhaps even *Ivan durak* 'John is a fool'+*Ivan durak* = *Ivan durak durakom* 'John's an awful fool', although such mechanisms should not be insisted on too much. It is this combination of prediction within predication that permits the addition of such degree modifiers as *sovsem* 'completely' to S^2_i, e.g. *Ivan priexal sovsem starikom* 'John arrived a real old man', whereas such modification is impossible in e.g. *Boris čital večerom* 'Boris read in the evening' → **Boris čital sovsem večerom*. There may be a connection between the possibility vs. impossibility of such degree modification and the derivational network of S^2_i: if, e.g., there exists for the given S^2_i the transformation T: S → A (*starik* 'old man' → *staryj* 'old') and for the resulting A the transformation T: $A_{pos} \rightarrow A_{comp}$ (*staryj* 'old' → *starše* 'older'), then one can add *sovsem* to the S^1_n V S^2_i unit (it is interesting to note that such degree modification is only possible at the extremes 'completely' and 'not at all'; although we have all degrees — *on sovsem star* 'he's quite old', *on dovol'no star* 'he's rather old', *on nemnožko star* 'he's a bit on the old side', *on otnjud' ne star* 'he's not in the least old' — we can derive only *on priexal sovsem starikom* 'he was quite an old man when he arrived' and *on priexal otnjud' ne starikom* 'he wasn't at all old when he arrived', but not **on priexal dovol'no starikom* 'he was rather an old man when he arrived' or **on priexal nemnožko starikom* 'he was a bit of an old man when he arrived'); such modification is impossible or unlikely in units for which no T: $S^2 \rightarrow$ A is possible (e.g. when S^2_i = *verenicej* 'in a row', *šopotom* 'in a whisper') or, if such T is possible, where no degree transformation T: $A_{pos} \rightarrow A_{comp}$ is possible (e.g. *večer* 'evening' (noun) → *večernij* 'evening' (adj.), but no *večernij* → **večernee*). Note that in the very similar S^1_n V S^2_a S^3_i units a case of constructional homonymity[48] obtains whenever the unit can be derived from two different sets of simpler units; e.g., the unit *Ivan znal Borisa studentom* 'John knew Boris as a student' can be derived from both *Ivan znael Borisa* 'John knows Boris'+*Ivan student* 'John is a student' and *Ivan znael Borisa* 'John knows Boris'+*Boris student* 'Boris is a student', and consequently only the context can tell us whether *studentom* refers to *Ivan* or to *Borisa*.

In a broader sense of the term, many other S^1_n V S^2_i units

[48] Cf. Chomsky, *Syntactic Structures*, 85f.

could be called predicative, since they too can be derived from pairs of simpler units, e.g. *oni šli lesom* 'they were walking through the forest' = *oni šli* 'they walked'+*oni v lesu* 'they are in the forest', *Boris čital večerom* 'Boris was reading in the evening' = *Boris čital* 'Boris was reading'+*bylo večerom* 'it was in the evening'; in none of these other cases, however, can the S^1_n V S^2_i unit be derived from two simpler units with identical S^1_n, which is the case with *Ivan priexal starikom* 'John arrived an old man' = *Ivan priexal* 'John arrived'+*Ivan byl starikom* 'John was an old man'. Cf. 1.131.

2.1212. Units in which T: → S^1_n byl- S^2_i is impossible, e.g. *ona vyla šakalom* 'she howled like a jackal' → **ona byla šakalom* 'she was a jackal', *oni šli verenicej* 'they went in a row' → **oni byli verenicej* 'they were a row', contain non-predicative modifiers, either comparative or metamorphic.

2.12121. Non-predicative units in which the transformation T: → S^1_n V kak S^2_n is possible contain comparative instrumental modifiers, which describe V rather than S^1_n, e.g. *ona vyla šakalom* 'she howled like a jackal' → *ona vyla kak šakal* (*kak* 'like, as'); note that T:→ S^1_n byl- kak S^2_n is not the same, e.g. *ona vyla kak šakal* = (i.e., can be transformed to) *ona vyla, kak vyl by šakal* 'she howled as a jackal would howl', not *ona byla kak šakal kogda ona vyla* 'she was like a jackal when she howled'.

2.12122. In certain non-predicative units there obtains, in addition to the comparative T: → S^1_n V kak S^2_n just mentioned, an additional, prepositional transformation T: → S^1_n V pS^2, e.g. *oni šli verenicej* 'they went in a row' → *oni šli v verenice* (*v* 'in'). Such units can be called metamorphic, since S^1_n, in performing the action V, takes on temporarily the form of S^2; in other words, in metamorphic units S^2_i characterizes neither S^1 alone (as in 2.1211) nor V alone (as in 2.12121), but S^1_n as engaged in V.

2.122. Units in which the transformation T: → S^2_n V is impossible are non-analogous, e.g. *oni šli lesom* 'they were walking in the forest' → **les šel* 'the forest walked', *Boris čital večerom* 'Boris read in the evening' → **večer čital* 'the evening read', *on govoril šopotom* 'he spoke in a whisper' → **šopot govoril* 'a whisper spoke'. They are divided into two groups, temporal-spatial and semi-tautological.

2.1221. Units in which some prepositional transformation T: → S^1_n V pS^2 is possible contain either temporal or spatial instru-

mental modifiers, e.g. *oni šli lesom* → *oni šli v lesu* 'in the forest', *po lesu* 'through the forest', etc., *Boris čitaet večerom* 'Boris reads in the evening' → *Boris čitaet pod večer* 'towards evening', *po večeram* 'in the evenings', *v ètot večer* 'this evening', etc. The further division into units containing temporal vs. spatial modifiers is made on the basis of formal features of S^2_i not directly connected with this paper, e.g. possibility or impossibility of modifying verbs in *-sja* by accusative substantives.

2.1222. Units in which no T: → S^1_n V pS^2 is possible, but for which on the other hand an interclass transformation T: → S^1_n V_{s2} (where V_{s2} is a verb derived from S^2) is possible, can be called semi-tautological, since the derived V_{s2} is a form of the action expressed by V, e.g. *on govoril šopotom* 'he spoke in a whisper' cannot* → *on govoril v šopote*, but can → *on šeptal* 'he whispered', and *šeptat'* is a manner of *govorit'*. There are very few such units, all of which correspond to the more frequent constructions with obligatory adjective modifier of S^2_i, e.g. *on govoril tixim golosom* 'he spoke in a quiet voice' (cf. 3.1f.).

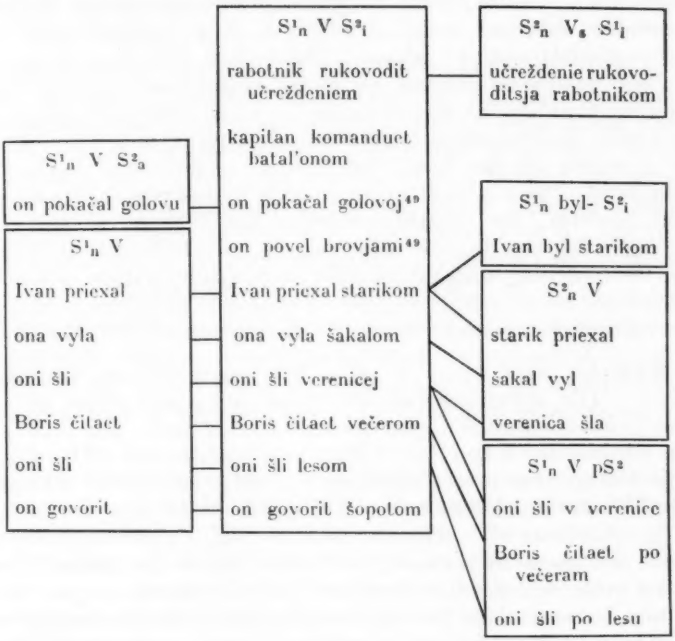
2.2. The transform features of S^1_n V S^2_i units discussed above can be summarized in tabular form as follows:

TABLE 2A: TRANSFORM NETWORK OF S^1_n V S^2_i UNITS

T: →	S^2_i ⊕	⊕ → A_i	S^2_n V_s S^1_i	S^2_n V	S^2_i → S^2_s	S^1_n byl- S^2_n	S^1_n V kak S^2_n	S^1_n V pS^2	S^1_n V S_2	(V_s S^2_s)	
	—	+	+	—	—	—	—	—	—	—	rabotnik rukovodit učreždeniem
	(—)	+	—	—	—	—	—	—	—	—	kapitan komanduet batal'onom
	—	(—)	—	—	+	—	—	—	—	—	on pokačal golovoj
	—	(—)	—	—	—	—	—	—	—	—	on povel brovjami
	+	+	—	+	—	+	—	—	(+)	—	Ivan priexal starikom
	+	+	—	+	—	—	+	—	—	—	ona vyla šakalom
	+	+	—	+	—	—	—	+	—	—	oni šli verenicej
	+	+	—	—	—	—	—	+	—	+	Boris čitaet večerom
	+	+	—	—	—	—	—	+	—	(—)	oni šli lesom
	+	+	—	—	—	—	—	—	+	—	on govorit šopotom

2.3. The network of correlated transforms in which units of type S^1_n V S^2_i participate can be represented schematically as follows:

TABLE 2B: TRANSFORM NETWORK OF S^1_n V S^2_i UNITS



3. PHRASE TYPE S^1_n V A_i S^2_i.

3.1. Units[50] of morphologic type S^1_n V A_i S^2_i (rarely, S^1_n V_s A_i S^2_i) are divided into two types, according to whether or not the adjective modifier of S^2_i can be omitted (formally, whether or not T: $A_i \rightarrow \emptyset$ is possible). Where A_i can be omitted (e.g,. *kapitan komandoval pervym batal'onom* 'the captain commanded the first batallion' → *kapitan komandoval batal'onom*, *student udarilsja ostrym nožom* 'the student struck himself with a sharp knife' → *student udarilsja nožom*), such omission produces units of

[49] Units characterized by many restrictions on T: $\emptyset \rightarrow A_i$; these restrictions cannot conveniently be represented schematically.

[50] Some of the transformations discussed in this section were first worked out in conversation with Professor Morris Halle of M.I.T. in 1955-56.

types S^1_n V S^2_i or S^1_n V_s S^2_i, of which the original unit with A_j must be considered an expansion. Units in which T: A → ø is impossible, however, form an entirely separate group, being in themselves minimal units, of which A_i is an integral part (e.g., *ona pogljadela svetlymi glazami* 'she looked with her clear eyes' → **ona pogljadela glazami*, *on govoril spokojnym tonom* 'he spoke in a calm tone' → **on govoril tonom*). In all such units there is an obviously close semantic connection between V and S^2_i. Unless the meaning of the term 'metonymy' is stretched beyond its usual limits, there is no established term to describe such a connection; however, the semantic relationship between V and S^2_i is so close that this type of unit can be called 'semi-tautological'. The instrumental substantive adds no new information of its own, but instead serves simply as a sort of syntactic middleman, enabling the information content of A_i to be introduced into the unit.[51]

3.11. All units where T: A_i → ø is impossible are predicative units. The difference between these units and those of type *Ivan priexal starikom* 'John arrived an old man' (cf. 2.1211) is that whereas the latter derive from two predications with common subject (e.g., *Ivan priexal starikom* = *Ivan priexal* 'John arrived' + *Ivan byl starikom* 'John was an old man'), the former derive from two predications with separate subjects (e.g., *on govoril spokojnym tonom* 'he spoke in a calm tone' = *on govoril* 'he spoke' + *(ego) ton byl spokojnym* 'his tone was calm'); there is always a synecdochic relation between these two subjects and hence also between S^1 and S^2 of the derived unit (e.g., *on govoril serditym golosom* 'he spoke in an angry voice', *Tat'jana dikimi glazami oziraetsja* 'Tat'jana gazes about with wild eyes', *ona gljadela bol'šimi glazami* 'she was looking with her big eyes'). This derivation of the S^1_n V A_i S^2_i unit from two simpler units is formally demonstrable by the transformation T: → kogda S^1_n V, A_p S^2_n byl- $A_{i'n}$ (where A_p is a possessive pronominal adjective *moj* 'my', *tvoj* 'thy', *naš* 'our', *vaš* 'your', or substantive *ego* 'his', *ee* 'hers', *ix* 'theirs' and *byl-* is any tense form of *byt'*), e.g. *kapitan smotrel ostorožnymi glazami* 'the captain looked with cautious eyes' → *kogda kapitan smotrel, ego glaza byli ostorožnymi* 'when the captain looked, his

[51] These units are closely related to units of phrase type S^1_n V S^3_i S^2_g, e.g. *on govoril tonom nastavnika* 'he spoke in the tone of a tutor'; the essential is that S^2_i be modified, and just how it is modified is a secondary matter. This is one of the cases where transformation classification cuts across the lines of, and even contradicts, morphological phrase-type classification.

eyes were cautious'. Various nonproductive sub-types of this transformation are possible, according to whether $A_{i/n}$ is in the short or long form (*svetly/svetlye*), nominative or instrumental (*svetlye/svetlymi*).[52]

3.12. That the instrumental adjective modifies the substantive S^2_i is obvious. It is less obvious, however, that this same instrumental adjective either does or can modify (by implication, due to the transform correlations into which the given unit enters), in addition, the verb V, the subject S^1_n, or both. In fact, all distinctions within the group of S^1_n V A_i S^2_i units are made on this basis, namely which of the other two items V or S^1_n the information content of A_i can and cannot be applied to. We will discuss the relation of A_i first to V, then to S^1_n.

3.121. Units of type S^1_n V A_i S^2_i can be divided into two groups, according to whether or not the information content of A_i can be applied to the verb V (formally, whether or not the transformation T: $\rightarrow S^1_n$ Aø V is possible, where Aø is the zero or adverbial form of A_i).

3.1211. Units in which the adverbial transformation T: $\rightarrow S^1_n$ Aø V is possible can be said to contain semi-adverbial instrumental modifiers. This is the case with the majority of S^1_n V A_i S^2_i units, e.g. *on kričal gromkim golosom* 'he shouted in a loud voice' $\rightarrow$ *on gromko kričal* 'he shouted loudly', *kapitan smotrel ostorožnymi glazami* 'the captain looked with cautious eyes' $\rightarrow$ *kapitan ostorožno smotrel* 'the captain looked cautiously', *on ušel bystrymi šagami* 'he went off with rapid steps' $\rightarrow$ *on bystro ušel* 'he went off rapidly'. Adjectives occurring in units for which this adverbial transformation is possible can be called (if a general term is needed) 'qualifiers', since they are specifically opposed to the 'visible quantifiers' discussed just below.

3.1212. Units in which the adverbial transformation T: $\rightarrow S^1_n$ Aø V is impossible are fewer than the units just discussed; they can be said to contain non-adverbial instrumental modifiers. In all such combinations the adjective A_i is what can be somewhat cumbersomely called a 'visible bi-polar quantifier', by which is meant that such an adjective measures its modified substantive as being at one or the other end of some visible scale, such as big—little, wide—narrow, long—short, high—low. Examples of such

[52] Cf. Morton Benson, "Predicate Adjective Usage in Modern Russion," to appear in *Word*, vol. 15 (1959), no. 1.

units are: *ona gljadela bol'šimi glazami* 'she looked with big eyes' → **ona veliko gljadela* 'she looked greatly', *on vzgljanul uzkimi glazami* 'he glanced up with narrow eyes' → **on uzko vzgljanul* 'he glanced up narrowly', *on ušel dlinnymi šagami* 'he went off with long steps' → **on dlinno ušel* 'he went off lengthily': such transformations are impossible even when an originally visible quantifier is used figuratively, for example 'high', 'low' referring to voice tone, e.g. *on govoril nizkim tonom* 'he spoke in a low tone' → **on nizko govoril* 'he spoke lowly', *ona otvečala vysokim golosom* 'she answered in a high voice'→ **ona vysoko otvečala* 'she answered highly'.

3.122. A second division of S^1_n V A_i S^2_i units is made according to whether or not the information content of A_i can be applied to the subject S^1_n (formally, whether or not T:→ S^1_n byl-A is possible).

3.1221. Units in which the transformation T: → S^1_n byl A is possible can be said to contain pseudo-predicative instrumental modifiers, in the sense that A_i rather implies the statement S^1_n A (e.g., *kapitan smotrel ostorožnymi glazami* 'the captain looked with cautious eyes' implies *kapitan byl ostorožen* 'the captain was cautious'), but does not make this statement outright; in fact, the implication can always be explicitly denied (e.g., *on govoril veselym tonom, xotja on sam vovse ne byl vesel* 'he spoke in a gay tone, although he wasn't in the least gay himself'). Examples of such units: *on govoril serditym golosom* 'he spoke in an angry voice' → *on byl serdit* 'he was angry', *on doložil uverennym golosom* 'he announced in a confident voice'→ *on byl uveren*, 'he was confident', *on skazal veselym tonom* 'he said in a gay tone' → *on byl vesel* 'he was gay', *on govoril spokojnym tonom* 'he spoke in a calm tone' → *on byl spokoen* 'he was calm'. This transformation is impossible for all units for which the adverbial transformation (cf. 3.1212) is impossible.

3.1222. Units in which the transformation T: → S^1_n byl-A is impossible imply nothing about the subject S^1_n and contain purely adjectival instrumental modifiers which describe only the semi-tautological instrumental substantive S^2_i, e.g. *ona pogljadela svetlymi glazami* 'she looked with her clear eyes' → **ona byla svetla* 'she was clear', *Gavrila tupymi glazami pogljadyval* 'Gavrila looked with dull eyes' → **Gavrila byl tup* 'Gavrila was dull', *on govoril nizkim tonom* 'he spoke in a low tone'* → *on byl nizok* 'he was low'. This group includes all units for which the adverbial transformation T: → S^1_n Aø V is impossible, e.g. *on ušel dlinnymi*

šagami 'he went off with long steps' → **on dlinno ušel* 'he went off lengthily' and → **on byl dlinen* 'he was long', *on vzgljanul uzkimi glazami* 'he looked up with narrow eyes' → **on uzko vzgljanul* 'he looked up narrowly' and → **on byl uzok* 'he was narrow'.

3.13. Another type of transformation, which can be called that of synecdochic inversion, is possible in certain cases (formally, T: → $A_n\ S^2_n\ V$ and variants thereof), e.g. *on doložil uverennym golosom* 'he announced in a confident voice' → *uverennyj golos doložil* 'a confident voice announced', *ona gljadit svetlymi glazami* 'she looks with her clear eyes' → *svetlye glaza gljadjat* 'the clear eyes look'. The possibility or impossibility of synecdochic inversions depends primarily on the particular lexical units involved, e.g. *on ušel bystrymi šagami* 'he went off with quick steps' can probably not → **bystrye šagi ušli* 'the quick steps went off', but the very similar *on udaljalsja bystrymi šagami* 'he moved off with quick steps' probably can → *bystrye šagi udaljalis'* 'the quick steps moved off'. Since synecdoche always remains a device, a deliberate aberration from normal speech, the acceptability of which is largely a matter of individual taste, it would probably be futile to seek structural rules underlying its use.

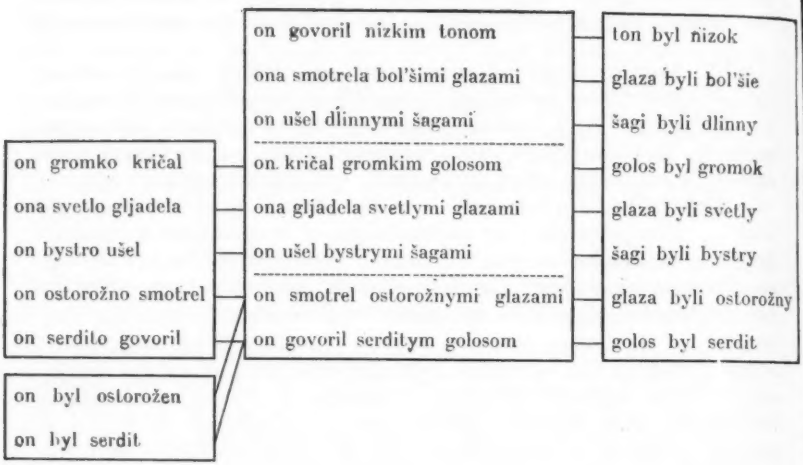
3.2. The transform features of $S^1_n\ V\ A_i\ S^2_i$ units can be summarized in tabular form as follows (synecdochic transformations are omitted):

TABLE 3A: TRANSFORM FEATURES OF $S_n\ V\ A_i\ S^2_i$ UNITS

T: → S^2_n byl- A	S^1_n Ao V	S^1_n byl- A	
+	+	+	kapitan smotrel ostorožnymi glazami, on skazal serditym golosom, etc.
+	+	—	on kričal gromkim golosom, ona gljadela svetlymi glazami, etc.
+	—	—	on govoril nizkim tonom, ona smotrela bol'šimi glazami, etc.

3.3. The network of correlated transforms into which units of type $S^1_n\ V\ A_i\ S^2_i$ enter can be represented schematically as follows:

TABLE 3B: TRANSFORM NETWORK OF S^1_n V A_I S^2_i UNITS



4. PHRASE TYPE S^1_n V S^2_a S^3_i.

4.1. Units of type S^1_n V S^2_a S^3_i are divided into two groups, according to whether or not the accusative direct object can be omitted (formally, whether or not T: $S^2_a \rightarrow$ ø is possible). Where S^2_a can be omitted (e.g., *on govoril èto šopotom* 'he said that in a whisper' → *on govoril šopotom* 'he spoke in a whisper', *Ivan čital knigu večerom* 'John was reading a book in the evening → *Ivan čital večerom* 'John was reading in the evening'), such omission produces units of type S^1_n V S^2_i, of which the original S^1_n V S^2_a S^3_i must be considered an expansion. Units in which such omission of S^2_a is impossible, however, are themselves minimal units (at least from the point of view of the present analysis, which deals only with instrumental modifiers; a full transform syntax would consider many S^1_n V S^2_a S^3_i units to be instrumental expansions of original S^1_n V S^2_n units; cf. 4.1233 below).

S^1_n V S^2_a S^3_i units for which T: $S^2_a \rightarrow$ ø is impossible (e.g. *ja sčitaju ego durakom* 'I consider him a fool' → **ja sčitaju durakom* 'I consider a fool', *on zakryl dver' rukoj* 'he closed the door with his hand' → **on zakryl rukoj* 'he closed with his hand') are divided into predicative and non-predicative units, according to whether

or not the unit posits an identity between S^2_a and S^3_i (formally, whether or not a transformation T: → $S^2_n \# S^3_n$ is possible.

4.11. Predicative units are those in which T: → $S^2_n \# S^3_n$ is possible, e.g. *oni vybrali ego prezidentom* 'they elected him president' → *on # prezident* 'he is president', *Petrovy nazvali syna Ivanom* 'The Petrovs named their son John' → *Ivan # syn* 'John is the son', *ja sčitaju ego durakom* 'I consider him a fool' → *on # durak* 'he is a fool', *ja znal ego studentom* 'I knew him as a student' → *on # student* 'he is a student'. There are a number of sub-types of predicative unit, all of which are highly restricted lexically.

4.111. Units of inceptive status contain verbs which themselves create the identity of S^2_a and S^3_i, e.g. *oni delali ego sekretarem* 'they made him secretary'; this inceptivity can be demonstrated by transformations containing a form of *stat'* 'become', e.g. *oni vybrali ego prezidentom* 'they elected him president' → *on stal prezidentom* 'he became president', etc. Personal names are a special case within this group, e.g. *Petrovy nazvali syna Ivanom* 'the Petrovs named their son John' → *syn stal (nazyval'sja) Ivanom* 'the son began to be called John'.

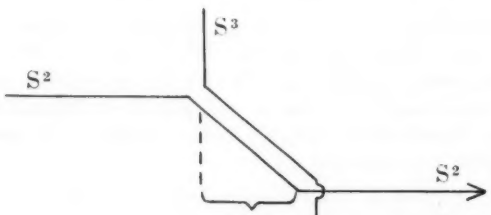
4.112. Pseudo-predicative units express a certain attitude on the part of S^1_n toward the predicative identity of S^2 and S^3, but this identity is not posited as truth, e.g. *ja sčitaju ego durakom* 'I consider him a fool', *druz'ja veličali (ego) statejku (učenym) trudom* 'his friends honored his little article with the name of scholarly opus'.

4.113. In temporal units the instrumental substantive S^3_i is predicated as identical to S^2_a during the time span in which the action V occurs, but only during this time, e.g. *ja znal ego studentom* 'I knew him as a student' (derived from *ja znal ego* 'I knew him' +*on byl studentom* 'he was a student', cf. 2.1211), *Ivan vstretil Petra (ešče) lejtenantom* 'John had already met Peter as a lieutenant', *my uvideli ego (snova) docentom* 'we saw him again as a young professor'.

4.12. Non-predicative units of type S^1_n V S^2_a S^3_i, for which no T: → $S^2_n \# S^3_n$ is possible, are the largest single group of units containing instrumental substantive modifiers, and contain what are generally if somewhat loosely referred to as "instrumentals of means," e.g. *on zakryl dver' rukoj* 'he closed the door with his hand', *ona nabila podušku puxom* 'she stuffed the pillow with down'.

Within this group there are two rather clearly opposed sub-groups, distinguished by the quite different relations between S^2 and S^3 expressed in the one and the other sub-group. These two sub-groups will be said to contain 'true instrumentals' on the one hand and instrumentals of 'resultant contiguity' on the other. These two subgroups will be described briefly in 4.121 and 4.122; the transformation features which distinguish the one from the other will be taken up in 4.123.

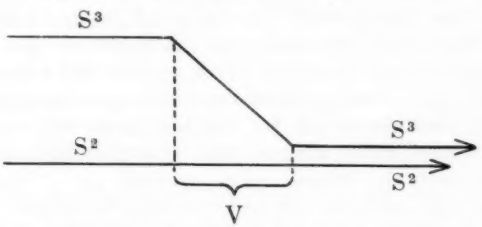
4.121. In units containing true instrumental modifiers, S^3_i is really the means or instrument by which S^1_n accomplishes the action V, e.g. *on zakryl dver' rukoj* 'he closed the door with his hand', *roditeli portjat detej balovstvom* 'parents spoil children by over-indulgence', *Ivan vyčerknul slovo karandašom* 'John crossed out the word with his pencil', *publika vstretila ego aplodismentami* 'the audience greeted him with applause'. In all such cases the relation between S^3 and S^2 is temporally limited to the duration of the action V; once the time span of V has passed, there is no further connection between S^3 and S^2. This temporally limited relation between S^3 and S^2 can be represented graphically as:



In other words, S^3 is intimately associated with S^2 during the time occupied by V, but this association ceases with the cessation of V. Other examples of true instrumental modifiers include *tetja vyzyvala smjatenie (ložnymi) spletnjami* 'my aunt caused confusion with her false gossiping', *on pričinjaet besporjadok (svoimi) šalostjami* 'he causes disorder with his pranks', *Ivan pugal menja bleſom* 'John scared me with his bluff', *kučer vzbodril kljaču (gromkim) ponukan'em* 'the coachman encouraged his nag with loud urgings-on', *on toptal pol sapogami* 'he got the floor dirty with his boots', *on obter lico platkom* 'he wiped his face with a

cloth', *soldat prikolol ranenogo štykom* 'the soldier finished off the wounded man with his bayonet', *on udivil menja otvetom* 'he astonished me with his answer'.

4.122. In units containing instrumentals of resultant contiguity, the action V itself establishes a relation of spatial contiguity between S^2 and S^3, and this contiguity continues indefinitely after the action of V has ceased, e.g. *rabočie pokryli ulicu asfal'tom* 'the workers covered the street with asphalt', *monax napolnil kuvšin vodoj* 'the monk filled his jug with water', *Ivan zakryl lico vorotnikom* 'John covered his face with his collar', *oni posypali rel'sy peskom* 'they sanded the rails'. The establishment of this spatial contiguity can be represented graphically as:



4.123. This difference in the relations between S^2 and S^3 expressed in units containing true instrumental modifiers and those containing modifiers of resultant contiguity finds formal expression in a number of transform features, some of which are obvious and almost absolute, others of which are only more or less clear tendencies.

4.1231. The most obvious and consistent formal feature of units containing instrumentals of resultant contiguity is the possibility of forming prepositional transforms T: → $pS^2\ S^3$, e.g. *rabočie pokryli ulicu asfal'tom* 'the workers covered the street with asphalt' → *na ulice asfal't* 'asphalt is on the street', *ona nabila podušku puxom* 'she stuffed the pillow with down' → *v poduške pux* 'there is down in the pillow', *on zakryl lico vorotnikom* 'he covered his face with his collar' → *pered licom vorotnik* 'the collar is before his face'. Such transformations are usually impossible for units containing true instrumental modifiers, e.g. *on zakryl dver' rukoj* 'he closed the door with his hand' → **na dveri ruka* 'his hand is on the door', *roditeli porljat detej balovstvom* 'parents spoil children by over-indulgence' → **u detej balovstvo* 'children have over-indulgence', *on vyčerknul slovo karandašom* 'he crossed

out the word with his pencil' → **na slove karandaš* 'on the word is a pencil', etc.

4.1232. The temporal limitation of the relation between S^3 and S^2 (to the time span during which the action V occurs) brings S^3 closer to the role of a subject in true instrumental units than in units with resultant-contiguity modifiers. This greater subjectivity is formally expressed in the relative ease with which such subjective transformations as T: → $S^3_n\ S^1\ V\ S^2_a$ are effected, e.g. *roditeli portjat detej balovstvom* 'parents spoil children by over-indulgence' → *balovstvo roditelej portit detej* 'the parents' over-indulgence spoils the children', *publika vstretila ego aplodismentami* 'the audience greeted him with applause' → *aplodismenty publiki vstretili ego* 'the applause of the audience greeted him', *on zakryl dver' rukoj* 'he closed the door with his hand' → *ego ruka zakryla dver'* 'his hand closed the door' (with special WO rules for original pronominal S^1_n). Such transformations are usually much more awkward, if possible at all, for combinations with instrumentals of resultant contiguity, e.g. *rabočie pokryli ulicu asfal'tom* 'the workers covered the street with asphalt' → (*)*asfal't rabočix pokryl ulicu* 'the workers' asphalt covered the street', *ona nabila podušku puxom* 'she stuffed the pillow with down' → (*)*ee pux nabil podušku* 'her down stuffed the pillow', *on zakryl lico vorotnikom* 'he covered his face with his collar' → (*)*ego vorotnik zakryl lico* 'the collar covered his face'. This feature, of course, is not one of absolute possibility vs. impossibility of a certain transformation, but rather a relative feature of greater or less ease of transformation (which may equal a higher or lower degree of grammaticalness).

4.1233. The lack of any temporal limitation on the relation established between S^3 and S^2 in units with instrumentals of resultant contiguity (in other words, the permanency and hence the importance of this newly established relation) makes S^3 itself more essential to the $S^1_n\ V\ S^2_a\ S^3_i$ unit than in the case of units with true instrumental modifiers. This relatively greater importance of S^3 in the former case is expressed in the difficulty with which S^3_i can be omitted from such units (formally, the quasi-impossibility of T: $S^3_i \rightarrow \emptyset$), e.g. *rabočie pokryli ulicu asfal'tom* 'the workers covered the street with asphalt' → (*)*rabočie pokryli ulicu* 'the workers covered the street', *oni posypali rel'sy peskom* 'they sanded the rails' → (*)*oni posypali rel'sy* 'they scattered the

rails', *on napolnil sunduk bel'em* 'he filled the trunk with laundry' → (*)*on napolnil sunduk* 'he filled the trunk'. Such omission of S^3_i is on the other hand almost always possible for units containing true instrumental modifiers, e.g. *on zakryl dver' rukoj* 'he closed the door with his hand' → *on zakryl dver'* 'he closed the door', *roditeli portjat detej balovstvom* 'parents spoil children with over-indulgence' → *roditeli portjat detej* 'parents spoil children', *on udaril menja palkoj* 'he struck me with a stick' → *on udaril menja* 'he struck me'. As was the case in 4.1232, this is a relative, not an absolute feature.

4.124. A particular sub-group of resultant-contiguity units obtains with a lexically restricted number of verbs expressing the physical transfer of S^3_i to a person (more rarely a creation by a person) S^2_a, e.g. *on nadelil menja podarkami* 'he showered me with gifts', *avtor snabdil knigu primečanijami* 'the author provided the book with notes', *žjuri nagradil ego premiej* 'the jury awarded him the prize', (expressing lack of such transfer) *tetja obdelila menja nasledstvom* 'my aunt deprived me of my inheritance'.

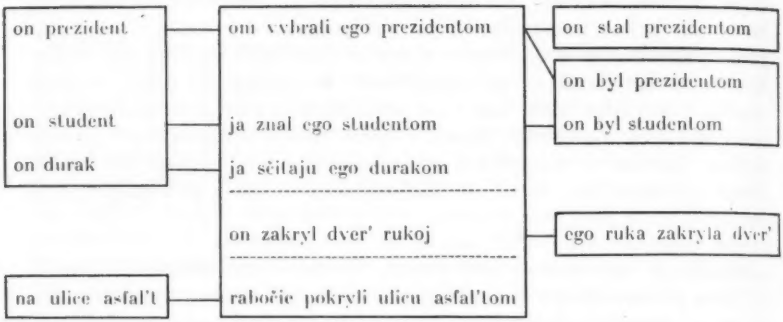
4.2. The transform features of S^1_n V S^2_a S^3_i units can be summarized in tabular form as follows (units in which S^2_a can → *ø* are omitted):

TABLE 4A: TRANSFORM FEATURES OF S^1_n V S^2_a S^3_i UNITS

T:	S^2_a ↑ ø	S^3_n # S^3_n	S^2_a stal- S^3_i	S^2_n byl- S^3_i	pS³ S^2_a	u S^2_x S^2_n	S^3_n S^1_x V S^3_x	S^3_i ↑ ø	
	—	+	+	+	—	—	—	+	oni vybrali ego prezidentom
	—	+	—	+	—	—	—	+	ja znal ego studentom
	—	+	—	—	—	—	—	—	ja sčitaju ego durakom
	—	—	—	—	—	—	(+)	+	on zakryl dver' rukoj
	—	—	—	—	—	—	+	+	on udivil nas otvetom
	—	—	—	—	+	—	(—)	(—)	rabočie pokryli ulicu asfal'tom
	—	—	—	—	+	+	—	—	tetja nadelila menja nasledstvom

4.3. The network of correlated transforms into which units of type S^1_n V S^2_a S^3_i enter can be represented schematically as follows (certain minor groups are omitted):

TABLE 4B: TRANSFORM NETWORK OF S^1_n V S^2_a S^3_i UNITS



5. PHRASE TYPE Vø S_i.

5.1. Units of type Vø S_i, e.g. *zateklo krov'ju* 'blood began to flow', *sverknulo rjab'ju* 'a ripple flashed', *xolodom pašet* 'there's a breath of cold', *paxlo osen'ju* 'it smelled of autumn', are formally characterized by the fact that V can occur only in neuter past or third person singular non-past (formally, the transformation T: Vø → V is impossible, e.g. *produvalo vetrom* 'a puff of wind blew' → **produvaem vetrom* 'we blow with the wind' or → **produvali vetrom* '[they] blew with the wind', etc.). There are two subgroups of type Vø S_i unit, according to whether or not a personal transformation T: → S_n V is possible, e.g. *zateklo krov'ju* 'blood began to flow' → *krov' zatekla*, but *popaxivaet dymom* 'it smells a bit of smoke' → **dym popaxivaet* 'smoke smells a bit'.

5.11. Units for which the personal transformation T: → S_n V is possible occur with a small number of verbs expressing physical and usually visible actions, e.g. *zateklo krov'ju* 'blood began to flow' → *krov' zatekla*, *produvalo vetrom* 'a puff of wind blew' → *veter produval*, *sverknulo rjab'ju* 'a ripple flashed' → *rjab' sverknula*, *skosilo gradom* 'the hail cut down' → *grad skosil*.

5.12. Units for which T: → S_n V is impossible occur with verbs expressing the transfer through the air of (a) an odor, e.g. *degol'kom potjanulo* 'there was a smell of tar', figuratively *paxnet vesnoj* 'it smells of spring' or (b) cold, damp or other touch-perceived sensation, e.g. *povejalo syrost'ju* 'there was a breath of dampness', *proxladoj dunulo* 'there was a puff of coolness'.

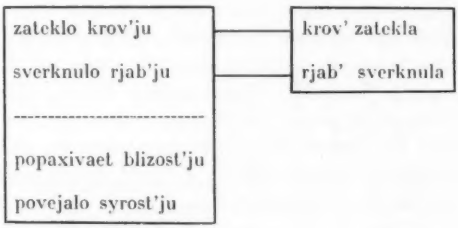
5.2. The transform features of Vø S_i units can be summarized in tabular form as follows:

TABLE 5A: TRANSFORM FEATURES OF Vø S_i UNITS

T: → Vø → V	S_n V	
—	+	zateklo krov'ju
—	—	popaxivaet dymom

5.3. The network of correlated transforms into which units of type Vø S_i enter can be represented schematically as follows:

TABLE 5B: TRANSFORM NETWORK OF Vø S_i UNITS



6. PHRASE TYPE S^1_a Vø S^2_i.

6.1. Units of type S^1_a Vø S^2_i, like the type Vø S_i units discussed in 5., are impersonal constructions characterized above all by the impossibility of changing the verb to a personal form agreeing with a subject, e.g., *šljapu uneslo vetrom* 'the hat was carried off by the wind' → **šljapu unesut vetrom* 'they will carry the hat off with the wind', → **šljapu unesla vetrom* 'she carried the hat off with the wind', etc. In some cases, however (specifically, where S^2_i refers to an object which can be at the disposition of human beings), a transformation to what might be called an anonymous construction (with subjectless third person plural verb form) is possible, e.g. *otca pereexalo avtomobilem* 'father was run over by a car' ('it ran over father...') → *otca pereexali avtomobilem* ('they ran over father...').

6.11. In all units of type S^1_a Vø S^2_i the personal transformation T: → S^2_n V S^1_a is possible, e.g. *otca ranilo oskolkom* 'father was wounded by a fragment' → *oskolok ranil otca* 'the fragment wounded

father', *lodku razbilo burej* 'the boat was smashed by the storm' → *burja razbila lodku* 'the storm smashed the boat', *luga zalilo vodoj* 'the meadows were flooded with water' → *voda zalila luga* 'water flooded the meadows'. Since all S^1_a Vø S^2_i units can be derived from S^2_n V S^1_a correlates, but not vice versa, the impersonal constructions must be considered derivations from the 'personals'.

6.12. In a few cases the S^1_a Vø S^2_i unit is characterized by the possibility of a further transformation T: → S^1_n V_s S^2_i, e.g. *luga (acc.) zalilo vodoj* ('the meadows it flooded with water') → *luga (nom.) zalilis' vodoj* 'the meadows were flooded with water' (cf. 1.114 above).

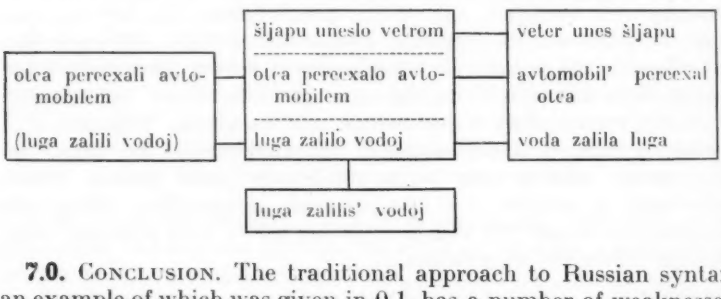
6.2. The transform features of S^1_a Vø S^2_i units can be summarized in tabular form as follows:

TABLE 6A: TRANSFORM FEATURES OF S^1_a Vø S^2_i UNITS

T: →	Vø	→ V	V →	
	S^2_u	$V S^1_a$	$S^2_u V_s$	$S^1_u V_s$
	—	+	+	—	šljapu uneslo vetrom
	—	+	+	—	otca pereexalo avtomobilem
	—	+	(+)	+	luga zalilo vodoj

6.3. The network of correlated transforms into which units of type S^1_a Vø S^2_i enter can be represented schematically as follows:

TABLE 6B: TRANSFORM NETWORK OF S^1_a Vø S^1_i UNITS



7.0. CONCLUSION. The traditional approach to Russian syntax, an example of which was given in 0.1, has a number of weaknesses, the most obvious of which is the absence of consistent classificatory criteria. Groups are described on the basis now of substantive

meaning, now of verb meaning, now of some combination of the two; the presence or absence of other modifiers, the degree of concreteness or abstraction of verb and substantive, the morphology of the verb itself (reflexive or not, passive participle or not), and the degree of semantic identity between verb and substantive are all determining factors in one or the other group. To the very large extent to which this traditional approach is based on meaning clusters alone, it suffers from further weaknesses. For one thing, given the enormous variety of individual word meanings and the difficulty of labeling these with precision, a classification based on groups of similar meanings must employ labels which are themselves very imprecise; to attain an interesting degree of generalization (i.e., to set up large enough classes), this classification must use labels of almost meaningless imprecision (e.g., the class label "objective" covers such variegated combinations as *paxat' traktorom* 'cultivate with a tractor', *nadelit' talantom* 'endow with talent', *ševelit' gubami* 'move one's lips', *poražat' krasotoj* 'astonish by one's beauty', *nabit' senom* 'stuff with hay', *ljubovat'sja prirodoj* 'admire nature', *upravljat' buksirom* 'run a tugboat').[53] Further, a classification of word-combinations based on the meanings of the words contained therein would seem dangerously circular, since the meaning of each word itself depends at least partly on its context, the most important part of which are those very words with which it is syntactically connected.

Perhaps the major flaw in the traditional approach, however, has lain in the fact that it has divorced meaning from form, and in so doing has departed from the realm of the demonstrable fact to enter that of the unprovable assertion. The discussion of differences of meaning and of semantic clustering is surely a fascinating endeavor, but as long as such discussion does not occur within some statable formal framework, it is hard to see how it can culminate in a convincing description. Transformation analysis provides this formal framework, using a classificatory procedure which is uniform throughout the entire analysis, and all the class labels and semantic interpretations of which are firmly grounded in demonstrable formal features. It substitutes for semantic generalizations a genuinely formal description; this is accomplished by expanding the concept of form itself and

[53] One suspects that this is a catch-all category consisting mostly of combinations which cannot be squeezed into the rubrics "temporal" and "spatial," the meanings of which are more homogenous and the formal characteristics of which more obvious.

by recognizing the existence of a different level of linguistic structure.

While the formal rigor of transformation analysis would by itself be a sufficient justification thereof, this approach has a number of further advantages. In some cases it produces more refined groupings, recognizing subtypes beyond the reach of traditional methods, e.g. the division of S^1_n V A_i S^2_i phrases into semi-adverbial and non-adverbial (*on kričal gromkim golosom* 'he shouted in a loud voice' → *on gromko kričal* 'he shouted loudly', but *on vzgljanul uzkimi glazami* 'he glanced up with his narrow eyes' cannot → **on uzko vzgljanul* 'he glanced up narrowly') in 3.121, or the establishment of the two categories "true instrumental" and "instrumental of resultant contiguity" (*on udivil menja otvetom* 'he astonished me with his answer': *oni posypali rel'sy peskom* 'they sanded the rails'). In several cases transformation analysis permits a type of sentence analysis impossible with older methods, for example the derivation of certain predicative constructions from combinations of two predications, e.g. *Ivan vernulsja starikom* ← *Ivan vernulsja*+*Ivan byl starikom* in 2.1211, and is apparently the only explanation of syntactic homonyms such as *ja znal ego studentom* 'I knew him as a student' (either 'when I was a student' or 'when he was a student'), cf. 2.1211, 4.113. It may provide additional syntactic characteristics of categories defined on other levels, e.g. the interrelation of perfectivity in verbs and animation in substantives expressed in the transform features of such phrases as *sčet sostavljaetsja buxgalterom* 'the account is made up by the bookkeeper', 1.112, or demonstrate the syntactic parallelism of phrases of quite different morphological structure, e. g. the impossibility of T: A → ø reductions in certain types of S^1_n V A_i S^2_i and S^1_n V S^2_i S^3_g phrase (*on govoril spokojnym tonom* 'he spoke in a calm voice' ≃ *on govoril tonom nastavnika* 'he spoke in the voice of a tutor'), 3.1, or the irrelevance of the presence or absence of *-sja* in such transformationally identical pairs as *Ivan priexal starikom* 'John arrived an old man' ≃ *Ivan vernulsja starikom* 'John returned an old man' or *on govorit tixim golosom* 'he speaks in a soft voice' ≃ *on vyražaetsja tixim golosom* 'he expresses himself in a soft voice', cf. Tables 1a, 2a. Further, transformation analysis provides the most consistent formal framework for describing whether or not certain types of modifier are obligatory (by giving a yes-or-no answer to the question whether, e.g., T: A_i → ø is possible for phrases like *ona govorila*

vysokim golosom 'she spoke in a high voice') as well as for describing restrictions on the type of modifiers which can be added to certain phrase types (by answering whether or not T: ø → A is possible, and if so, for which classes of A, etc.). Although deliberately chosen to avoid non-formalized semantic generalities, it may even suggest the existence of new semantic categories, e.g. the "bipolar visible quantifiers" of 3.1212. Even where a classification by transformation features produces groups identical to those of traditional classifications, it provides those groups with specific formal characteristics, e.g. the restrictions on T: ø → A transformations in the group containing *on pokačal golovoj* 'he shook his head', 2.1111; the fact that there are a number of such cases suggests that the traditional semantic classifications were in part based on formal features unrecognized at that time and perhaps unrecognizable except through transformation analysis.

7.1. TRANSFORM POTENTIAL. Perhaps the greatest single advantage of an analysis in terms of possible and impossible transformations is that it reveals the existence of a level of linguistic form superior to that of mere morphological description. It has been shown that within each morphologically defined phrase type there exist from a few to several transformationally defined sub-types, each of which is characterized by a particular set of transformations. The possibility of being transformed to all and only the members of a particular set of correlated phrase types can be called the transform potential of a sub-type. This potential is inherent in the sub-type and is as much a formal characteristic thereof as, say, the fact of belonging to a particular set of correlated morphemes is characteristic of membership in a certain word class. Each of the individual transformational possibilities or impossibilities which make up the total potential can then be termed a distinctive feature of transform potential ("distinctive," of course, because one such feature is enough to distinguish between otherwise identical sub-types). As transformational analysis uncovers the sub-types of all morphological phrase types in Russian, there will probably appear certain transformations of fundamental importance, whereas others will be seen to be of secondary or even redundant nature. Only after such a complete analysis has been effected and the set of basic kernel phrases and fundamental transformations established will it be possible to begin building up a complete syntax of Russian. This complete syntax will have to describe (1) a set of minimal sentence

types and (2) a set of transformations by which these minimal types can be expanded (T: ø → F), altered (T: F → F′), and combined (T: F+F′ → F″) to form the actual sentences possible in the language. The present discussion is offered as a step in the direction of such a syntax.

University of California at Los Angeles.

VOWEL HARMONY IN CLASSICAL MONGOLIAN[1]

ERIC P. HAMP

Because of the orthographic ambiguities of Classical Mongolian, one must not attach overly great importance to the phonemic analysis here offered. Nevertheless, in order to proceed, it is necessary, if only as a temporary measure, to make certain decisions. There is no need to dwell here upon considerations of the non-equivalence of graphemics with phonemics.

Basing ourselves on the texts to which I have had access,[2] we group in complementary distribution the sets which are often transliterated [a o u q γ] and [e ö ü k g], since their members do not occur mixed between spaces. From the latter set, in my 1948 analysis, I extracted a component (in the sense of Z. S. Harris, *Methods*, chapter 10; not, be it noted, a 'suprasegmental' feature) which I symbolized /ɐ/. This enabled me to phonemicize both sets (leaving the component aside) /a o u k g/. Though not decisive, it is worth noting that the Uigur-based script practices closely support such an analysis—ideas rarely come out of thin air. It might be conveniently claimed that the component extracted from the back series was compactly symbolized by absence of a sign, but that is not what I recall as my notion at the time. I think I simply extracted componential fronting and regarded

[1] In December 1948, in an oral paper to the Linguistic Society of America, I presented a descriptive sketch of the morphology of Classical Mongolian (ca. 1400 AD). In the handouts for that paper I listed at the beginning the phonemes that I assumed. I later had the opportunity to discuss that analysis, amongst other things, with great profit with Shirô Hattori. I find (*Word* 13.67, 1957, fn. 5) that I am credited, on the authority of a reference to Hattori, (with the judicious adverb 'Apparently') with a view on Mongolian vowel harmony. Since I have never published anything on the subject, and since (unwittingly, I am sure) Stuart and Haltod have placed my presumed view in a quite irrelevant context —their subject is Modern Standard Mongolian—I am prompted to return to the subject.

[2] It is a pleasure here to acknowledge the constant help I received from Francis Cleaves, of Harvard University, who introduced me to Mongolian and placed at my disposal many otherwise inaccessible texts.

the back series as lacking that feature. I cannot now defend that position, since it is clearly arbitrary not to extract backing instead, and regard fronting as absence of the former. In short, for Classical Mongolian, if any component at all is extracted, it must be both front and back /F B/.

Since we have in the Classical written sources almost no direct evidence for a 'culminative accent' and since there is, however, some evidence for juncture, we do best to base our formulation of the domain of the component(s) on juncture: The component holds force until the next juncture. The only real graphic evidence that I know of for a break in harmony is in the case of compounds, usually names; here I assume an intervening juncture: [esen buqa] becomes /^{F}asan+ ^{B}buka+/[3].

There are quite a number of cases where Mongol script practice leaves spaces before morphologically bound forms. In many of these instances, I arbitrarily posit phonemic close-juncture where a strictly phonemic decision is not justified on synchronic grounds, though in most such cases we have fair diachronic evidence. Thus it may be that my view produces texts with too few junctures, but the broad lines of the argument are not thereby affected.

The principal shortcoming that may be alleged for this phonemicization is the sin of overlapping allophones: One other range of vocoids occurred in the language, namely [i], and so far as we can tell from the script it occurred in both harmonies. Thus, in the case of certain monosyllables, e.g. [bi] 'I' it is methodologically illicit, if such overlap is outlawed in phonemic analysis, to assign either component.

Hence, observing the caveat on overlapping allophones, we must say that if my analysis of 1948, as now recapitulated, is otherwise justified, it applies not to recorded Classical Mongolian of post-1225, but rather to slightly prehistoric Mongolian of the period when [i] and [ï] had not yet fallen together. Historic Mongolian forms, as I was then writing them, would thus be morphophonemic rather than phonemic.

Alternatively, if Chomsky (*Syntactic Structures*, 1957) and Lees (*Language* 33.389, 1957) are right in rejecting bi-uniqueness,

[3] Stuart and Haltod remark (67) 'It is decidedly wrong to think of vowel harmony as a kind of euphony; Mongol speakers have no difficulty in pronouncing words in which vowel harmony is contradicted.' If, as it would seem from the sequence of their sentences, the authors think that such a notion of 'euphony' was in any way involved in or important to my view, I must correct that impression.

then my analysis still stands as serviceable in all pertinent respects. That is, although not quite all instances of /B/ and /F/ could be predicted unambiguously from the phonetic stream (as in the case of [bi] 'I'), the phonetic stream (pro forma, of course, since this is a dead language) could in fact be generated from the record of instances of the phonemes (including /B/ and /F/).

This system which I put forward in 1948, as modified by the above remarks, is then:[4]

V	i						
	u	o	a				—noteworthy allophones with /B F/
C	b	d	g	j			
	(p)	t	k	c			
	m	n	ŋ	s	(š)		
	r	l					
	w			y			

Word-components: B F +

TEXT

The following brief excerpt comprises lines 3 and 4 of the Sino-Mongolian inscription of 1362 (see F. W. Cleaves, *Harvard Journal of Asiatic Studies* 12.62 and 83, 1949). Junctures are indicated by spaces. It may be that the lack of voicing assimilation shown in clusters is morphophonemic rather than phonemic; it is hard to be sure on this point.

Fmoŋka Ftaŋriyin Fibagalibar Fyaka Bmoŋgol Bulusun
'eternal heaven's by-protection, great Mongol people's

Bkani Bnarbaiyi Fnigadkagulun Bjayagadagsabar
qan all (acc.) uniting whereas-he-was-successful-by-predestination,

Fdalakaiyin Fajan Fciŋgis Bkagan Ftaŋriyin Bjorigi
earth's master Cinggis Qagan heaven's will (acc.)

Bdagan Fumadu Bgajaraca Fyaka Fuilayi Fbutugan
following, northern from-land great affair (acc.) accomplishing

[4] Items graphically reflected only in loanwords are in parentheses.

Baltan Fbayabar Bulus Bkamun Bayalan Byabugsan
gold by-body nation (acc.) uniting, campaigning having-gone
Fdotugar Bondur Buigudun Bkan Binu Bbarcug Bard
4th in-year Uigurs' qan-the Barcuq Art
Bidug Bkud Fmadalun Firganiyan Babugad Boroju/
the-Iduq Qut domain's his-people (acc.) having-taken-with
submitting...'

University of Chicago.

THE LABIAL CONSONANTS OF PROTO-AFRO-ASIATIC

JOSEPH H. GREENBERG

Excluding *w* from consideration, almost all languages of the Afro-Asiatic family have three labial consonants, a nasal /*m*/, a voiced stop /*b*/, and a single unvoiced consonant, in some languages the unvoiced stop /*p*/, in others a labio-dental fricative /*f*/. Thus in Semitic, which may be taken as a representative example of the family as a whole, there are three original labial consonants, an **m* which appears everywhere in this form, a **b* which is found in all languages as /*b*/ and a single unvoiced consonant which is usually written **p*. This latter occurs as /*f*/ in all South Semitic languages (Ethiopian, North and South Arabic) and as /*p*/ elsewhere. In Hebrew and Aramaic a distinction between *p* and *f* developed at a later stage based on an originally non-phonemic alternation found also in the other non-emphatic stops, according to which a fricative allophone arose in postvocalic position while the stop was retained elsewhere.

An example of a parallel situation in a non-Semitic language of the family is Berber. Here again there are three labials, /*m*/, /*b*/, and /*f*/, corresponding to Proto-Semitic **m*, **b*, and **p* respectively.

Egyptian, however, has four distinct labial phonemes, /*m*/, /*b*/, /*p*/, and /*f*/. As might be expected, the first two correspond to Proto-Semitic **m* and **b* while the last two correspond to Proto-Semitic **p*. Those who have considered this matter have tacitly or explicitly assumed that Egyptian has innovated here, splitting an original single unvoiced labial into /*p*/ and /*f*/, although a conditioning factor for this change has never been discovered.[1] In view of the possible effects of vowels, which in the present state of knowledge cannot be reconstructed with

[1] A representative opinion is that of Marcel Cohen. "La solution adoptée ici est l'existence d'un seul phonème, l'argument principal étant que *p* et *f* correspondent suivant les mots au phonème unique du sémitique ; la division en deux phonèmes

certainty for earliest Egyptian, or in view of other unknown, possibly prosodic factors, and particularly in the absence of this distinction in the other languages of the family which have been reasonably well described up to now, this is a justifiable decision.

It is clear, however, that if some language or group of languages does exist in Afro-Asiatic which distinguishes two unvoiced labials and if these sounds correspond etymologically to the two unvoiced labials of Egyptian, then the above interpretation will have to be abandoned and it will be Semitic rather than Egyptian which will have innovated in merging two originally distinct labial phonemes.

It is the thesis of this paper that such a group of languages does exist, the subgroups of Chad languages spoken on the Jos Plateau in Nigeria, which will be called here the Plateau subgroup. The reconstruction of the original consonantal system of this group not only suggests with a high degree of probability that the Egyptian distinction between /p/ and /f/ is original but suggests the possibility of a still more complex labial series in Proto-Afro-Asiatic with similar implications for other series of consonants not considered in detail here, i.e. the dental and velar series.

The data on the Plateau group and other Chad languages presented here are partly based on my own field investigation and partly on other sources.[2] The chief Plateau languages utilized are Angas, Chip, Sura, Ankwe, and Gerka. The languages most closely related to the Plateau subgroup are the Bolewa group of languages, likewisse spoken in Nigeria, which includes Bolewa proper, Karekare, Ngamo, Kanakuru and Tangale.[3] Other Chad

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serait secondaire en égyptien, s'étant opérée dans des conditions qui nous échappent." (*Essai comparatif sur le vocabulaire et la phonétique du Hamito-Sémilique*, Paris, 1947, pp. 166-7).

[2] This material was gathered in the course of a linguistic survey of the Plateau and Bauchi provinces of Nigeria as a Fellow of the Guggenheim Foundation during the period of October 1955-August 1956. I am indebted to the Social Science Research Council and the Columbia Council for Research in the Social Sciences for supplementary support in connection with this research.

[3] Citations of Angas, Chip, Sura, Ankwe, Gerka and Bolewa forms are based on my own investigations unless otherwise indicated. Through the courtesy of the Roman Catholic authorities in Jos, I was permitted to consult the unpublished work of Father Sirlinger on Angas and Ankwe. Both of these contain far more extensive vocabularies than I was able to gather. A further source for Angas is H. D. Foulkes' *Angass Manual*, London (1915) which likewise has a very extensive vocabulary. Neither of these writers investigated the tonal systems of these languages but otherwise their work appears reliable and has been reinterpreted here in phonemic terms. Citations from Sirlinger are indicated by (S), from Foulkes by (F). In one instance

languages, particularly Hausa, Logone, Musgu and Mubi are also occasionally cited.[4]

The Plateau group is characterized in general by monosyllabic morphemes of the form CVC and VC as against CVCV and VCV of the Bolewa group and the remainder of the Chad languages in general. A typical instance is Plateau Angas, Chip, Sura *sár* 'hand' as against Bolewa *sārā*. In all of the Plateau languages thus far recorded and in Proto-Plateau as it can be reconstructed from these data, the number of distinct consonants which can occur as the final element of the common CVC morpheme type is limited to *p*, *t*, *s*, *k*, *m*, *n*, *ŋ*, *r* and *l*.[5] As might be expected, Plateau final *p* corresponds to any non-nasal labial of the non-Plateau languages and *m* corresponds to *m*. Thus Angas (Plateau) *rōp* 'to divide' is related to Hausa *rábà* 'to divide, separate', Angas *lōp* 'black' to Hausa *dúfù* 'darkness' and Angas *kwōm*, 'ear' to Bolewa *kūmō* 'ear'. In similar fashion final *n*, *ŋ*, *r* and *l* correspond to these same consonants elsewhere. However, final *n* frequently corresponds to the *l* of non-Plateau languages. All sibilants are reflected by *s*, velars by *k*, and dentals by *t*, except that the implosive *d* becomes *r* not *t* in the Plateau languages. Examples of all these will be found in the etymologies cited later in this paper.

In initial position, the Plateau languages have usually shifted the voiced non-imploded stops of the non-Plateau Chad languages and the other Afro-Asiatic languages to the corresponding unvoiced forms. The labials participate in this general shift so that in a number of instances Plateau *p* corresponds to *b* elsewhere. Examples of this sound change are:[6]

(1) (a) Ankwe *pìn*, Gerka *pìn* (b) Bolewa *bìn* 'room, hut', Kanakuru *mina* 'house', Sokoro *be:ni* 'to build' (c) Proto-Semitic **bny* 'to build', Cushitic Sidamo *min'* build', *minē:* 'house'

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a form from Ankwe is quoted from the work of J. F. J. Fitzpatrick, "Some Notes on the Kwolla District and Its Tribes," *Journal of the African Society*, 10.16-52, 213-21, 490ff. (1915).

[4] For a virtually complete bibliography of the Chad languages see M. A. Bryan and Dietrich Westermann, *Languages of West Africa*, Oxford (1952).

[5] In a few instances final *s* is found in Sura.

[6] Forms cited under (a) are from Plateau languages, under (b) from Chad non-Plateau languages and under (c) from branches of Afro-Asiatic other than Chad.

(2) (a) Angas *pār*, Sura *pār* 'night' (b) Bolewa *bōdì*, Ngamo *bɛdi*, Kanakuru *biri* 'night' (c) Cushitic Saho *bar* 'night'

(3) (a) Chip, Ankwe *pìł*, Gerka *pɔ̀ł* 'monkey' (b) Bolewa *bīdò*, Hausa *birì* 'monkey'

(4) (a) Angas, Chip, Sura *pò*, Ankwe *pùɔ̀*, Gerka *pàk* 'mouth' (b) Bolewa *bò*, Hausa *bà:kí:* 'mouth'[7]

For all Plateau languages from which data at present are available on this point, with the exception of Ankwe, the unvoiced stops which result from this shift merge with the earlier unvoiced stops which correspond to those of the remaining Chad languages and Afro-Asiatic in general. Ankwe, however, alone among the languages thus far studied, has a phonemic distinction between glottalized and non-glottalized unvoiced consonants. The non-glottalized stops issue from the older voiced stops as in those of the etymologies just quoted in which Ankwe figures. The original unvoiced consonants are preserved in Ankwe in glottalized form. This may be exemplified from the labial series by the following example:

(1) (a) Ankwe *p'ás*, Sura *pās*, Chip *pàs* (b) Bolewa *púzō* 'rainy season'

(2) (a) Ankwe *p'él*, Angas *pūł*, Chip *púł/pwáł*, Sura *pūł*
(b) Bolewa *pēlé*, Hausa *fìtá* 'to go out'

There is the further complication among the labial consonants, not paralleled in the other series, that for the single unvoiced labial of non-Plateau languages we find not only /*p'*/ in Ankwe and /*p*/ in languages other than Ankwe but a labiodental fricative /*f*/ in all the Plateau languages. An instance in point is the verb 'to boil' for which we have Angas *fīł*, Chip *fiál*, Sura *fīł*, Ankwe *fiál* as cognate forms to Bolewa *pūlō*.

Further, there are some instances in which the Plateau languages do not shift original *b* to *p* but instead preserve the voiced consonant. This is paralleled by instances of retention of original *d* and *g* in the other series. An example of this is Angas *bí*, Chip *bìì*, Ankwe *bìì* 'thing' related to Hausa *ʔàbù*, pre-genitival *ʔabi-*. Though not as frequent as the cases in which the change *b*>*p* occurs, there are a fair number of instances of such preser-

[7] I cannot say whether the two variants seen in the Bolewa and Hausa are etymologically related. There is a common afformative suffix *iki* in Hausa but I am not certain if its occurrence in other Chad languages.

vation of the voiced form. As yet no rule seems formulable by which the conditions for the presence or absence of this shift can be stated. We will call that *b* which becomes *p* in the Plateau languages $*b_1$ while that *b* which remains will be labelled $*b_2$.

Finally, the Chad languages in general distinguish in the labial and dental position between ordinary voiced and imploded voiced consonants, a distinction not found in any other branch of Afro-Asiatic. The voiced implosives of non-Plateau Chad languages are regularly reflected by similar sounds in the Plateau languages and it is found that the implosives are never devoiced. Examples in the labial series are relatively rare and those for related forms in other languages of the Chad sub family for which the distinction between *ɓ* and *b* has been reliably reported are even fewer. We may cite the example of Angas *ɓur* (S) 'ashes' which is probably cognate with Bolewa *ɓūtó*, and Gerka (Plateau) *ɓəl* 'horn' which is surely related to Bolewa *ɓālū*, Tangale *ɓwàl*.

We thus reconstruct for Proto-Plateau six labial consonants including *m*. In the following table Angas is taken as representative of the Plateau languages outside of Ankwe.

	Angas	*Ankwe*	*Proto-Chad*	*Proto-Semitic*
1.	f	f	*f	*p
2.	p	p'	*p	*p
3.	p	p	$*b_1$	*b
4.	b	b	$*b_2$	*b
5.	ɓ	ɓ	*ɓ	*b
6.	m	m	*m	*m

The hypothesis, then, with which we will be mainly concerned here is whether the distinction between Proto-Chad **f* and **p*, both of which appear in Semitic as **p*, coincides etymologically with Egyptian **f* and **p*. Since, as has been seen, the distinction of all labial non-nasals is lost in final position in the Plateau languages, we can only make comparisons in initial position. Since the relationship between the Plateau subgroup of Chad and Egyptian is fairly distant, we cannot expect a large number of etymologies involving the not very frequent unvoiced labial. We find five etymologies with initial Plateau *f* in agreement with Egyptian *f* and three with initial *p* corresponding to Egyptian *p*. In considering these etymologies, in addition to the consonantal changes in final position in the Plateau languages, we must also take into account the common correspondence of Egyptian ?

in non-initial position to *r* in Semitic and elsewhere and the total loss of the laryngeals ʔ, ʕ, *h*, and *ḥ* in the Plateau languages.

With *ʃ* we have the following: (1) Angas *ʃíír*, Chip *ʃér*, Sura *ʃéér*, Ankwe *ʃéér*, Egyptian *ʃdw*, Coptic (Sahidic) *ʃtów* 'four'. In Bolewa we find *pōɗɗō* and Hausa *fūɗū*. This is a basic Afro-Asiatic etymology with related forms likewise in Cushitic (Bedauye *faḍig*, Somali *afar*, Saho *afar*, Galla *afur*) and in practically all the remaining Chad languages. The change Proto-Chad *ɗ* > Plateau *r* in this position is regular as was pointed out earlier and exemplified in the word for 'night'. (2) Angas *ʃier* 'largeness', Egyptian *ʃʔ.w* 'power, authority'. Here *-w* is a singular abstract suffix in Egyptian and ʔ for Plateau *r* is regular. This etymology is therefore formally acceptable. Its weakness is the apparent absence of related forms in any other language. (3) Angas *ʃut* (S.), Ankwe *ʃuut* (S.) 'to vomit', Egyptian *ʃt* 'be disgusted' (Erman-Grapow 'sich ekeln'). This is phonetically straightforward and semantically plausible. There is, however, some possibility of sound symbolism. (4) Angas *ʃi*, Sura *ʃìì*, Ankwe *ʃìì*, Gerka *ʃ:*, Egyptian *ʃʔ* 'to blow'. Here again, with the usual Plateau loss of laryngeal, we have a formally and semantically acceptable etymology, though with some possibility of sound symbolism. (5) Angas *ʃīl*, Chip *ʃiál*, Sura *ʃīl*, Ankwe *ʃiál*, Gerka *ʃəl*, Egyptian *ʔʃr* 'to boil'. This is another fundamental Afro-Asiatic etymology. The root also occurs in Cushitic Galla *afel* and Berber Shilḥ *ʃufu* with partial reduplication, both meaning 'to boil'. Egyptian has no written *l*, though it must have existed since it appears later in Coptic. Forms with *l* in related languages are written often with *r*, sometimes with *n*. Semitic here shows related forms with *r*, e.g. Arabic *ʃwr*, Syriac *ʃr'*, *ʃwr*, Ethiopic *nfr* 'to boil'. Sporadic *r-l* correspondences are common in Afro-Asiatic. A further possible etymology is Angas *ʃél*, Sura *ʃél*, 'to sweep', Egyptian *ʃd* 'to wipe'. These were the Angas and Sura forms which I elicited. However, Sirlinger has both these forms and Ankwe *ʃal* 'to wipe, to sweep', Angas *ʃel* 'broom, to sweep'. There were then apparently doublets with initial *ʃ* and *ʃ*.

Correspondences involving *p* are: (1) Angas *piar* (S) 'to jump to leap', Ankwe *p'aar* (S) 'to jump', Egyptian *pʔ* 'to fly'. Here Egyptian ʔ corresponds to *r* elsewhere as noted earlier. The semantic transition is shown in Buduma (Chad) *fər* which has the meanings 'to jump, to dance, to fly'. There is also the probably related Angas *pir* (S) 'to stretch the wings'. This is a widespread

Afro-Asiatic root. Other examples are Cushitic Bedayue *fār* 'to hop, to jump', Berber Shilḥ *firri* 'to fly'. The common Semitic root 'to flee', Arabic *frr*, Syriac *prr* is doubtless likewise connected. (2) Angas *pūs*, Chip *pùs*, Sura *pūs* 'to shoot, to sting', Angas *pās*, Chip *pás*, Sura *pās* 'arrow'; Egyptian *pzḥ* 'to bite, to sting'. The Plateau loss of the laryngeal *ḥ* and change *z*>*s* are regular. Unfortunately, the absence of this form in Ankwe does not permit us to decide for Proto-Plateau *p* as against b_1 on Plateau evidence alone. (3) Angas *pok* 'to peel, to strip' (F), 'to remove grains from the stalk' (S); Egyptian *pg?* 'shell'. Here the Hausa *fíɡà* 'to strip a guinea corn stalk' gives welcome evidence that we are dealing with **g* and not **k*.

There are no plausible etymologies, of which I am aware, that involve Plateau *p* and Egyptian *f* or Plateau *f* and Egyptian *p*. Although, as has been noted, not all the above etymologies are equally likely, their combined effect is such that the correspondence of Plateau *p* and *f* to Egyptian *p* and *f* is highly probable; if this is so, it follows, that this distinction is original in Afro-Asiatic.

Regarding **b*$_1$ and **b*$_2$, there is some evidence that the distinction is proto-Afro-Asiatic and that **b*$_2$ may have been a prenasalized voiced stop, */mb/. The word for 'thing', quoted earlier, Angas *bi*, Chip *bii*, Ankwe *bìì* appears in Logone, another Chad language, as *mbi*. Further, if it is Angas *be* (S) 'side, direction', Ankwe *be* (Fitzpatrick) 'place' which is to be connected with Egyptian *bw;* Cushitic Sidamo *ba'a*, Gudella *beyo*, rather than Angas *pí*, Ankwe *pèé*, then we have once more a correspondence with Logone *mb* in *mba* 'place'. Further, Egyptian *bw* appears later in Coptic as *ma*. Finally, there is Angas *būl*, Chip *búl*, Sura *ñbūl*, Ankwe *bél* 'dove' which occurs in Bolewa as *mbōlè* and is related to Egyptian *mnwt* 'dove'. The final *t* in Egyptian is the feminine singular suffix.

As against this evidence we have a number of cases without *m* or *mb* in non-Plateau languages. An example is Angas *bś*, Chip *bís*, Sura *bíš* 'bad', related to Cushitic Quara *bisā* Sidamo *buššo* and found in Semitic as **b*?*s* (e.g. Arabic *bi*?*sa* 'it is bad', Akkadian *bīšu* 'bad'). The variants with *m* in the forms connected with Ankwe *pìn* 'room, hut' cited above in a form which goes back to initial **b*$_1$ is presumably due to assimilation to the following nasal.

Regarding the distinction between *ɓ* and *b*, for which there is at present no support outside of the Chad languages, it is safer to

assume, until such evidence is forthcoming, that they both continue a single original Proto-Afro-Asiatic phoneme. This conclusion is based on the same methodological considerations which justified the inference that the difference between *p* and *f* in Egyptian was secondary until the contrary thesis became highly probable because of the evidence presented in this paper.

Columbia University.

REMARKS ON THE HISTORICAL PHONOLOGY[1] OF AN EAST MEDITERRANEAN ARABIC DIALECT

IRENE GARBELL

0. *Introduction.*

0.1. While engaged in a study of the descriptive phonology of the Arabic dialect spoken in Jerusalem, the author of this paper has been confronted with several structural problems, the solutions of which might seem arbitrary if presented merely as rules within the framework of a synchronic description. These problems arise from the difficulty inherent in the analysis of any language from a purely static point of view. The forming of a language is a continuous process, albeit a slow one, and any given stage thereof necessarily reflects, besides the overall patterns of the present, several remains of the past, as well as indications of possible trends of development in the future. It has therefore seemed indicated to enquire into the different stages of development of this dialect (and incidentally of other cognate dialects, see 0.3), which can be assumed with a reasonable degree of certainty to have evolved from a common source.

0.2. The only diachronic description of an Arabic dialect to date (that of Lower Egypt)[2] is based upon the assumption that all Arabic dialects have developed from the Arabic *koiné* of the first centuries of the Islamic era. However, this *koiné* or common intertribal language of the Arab conquerors has been subjected to deep-going changes through contact with the native populations of the conquered areas.[3] This is particularly true of the period following the dissolution of the Abbasid empire in the 9th century, when local dialects were formed which were strongly influenced by the languages formerly spoken in the

[1] This term is used here in its broader connotation, comprising both phonemics and prosodics, cf. Martinet (1949): 10.

[2] Birkeland (1952).

[3] Fück: *passim*.

various regions — in the case of the Eastern provinces by Aramaic.[4] It would, however, be entirely erroneous to assume that the influence of Aramaic on the formation of certain Arabic dialects was strongest in earlier times and gradually weakened in subsequent periods. Certain indications in various literary sources show that the reverse is true, and this is far from being surprising. In the case of an entire population adopting a new language without the aid of accelerating factors present in a modern community—whether of an educational nature (such as compulsory school attendance for children, language classes for adults, the press, the theater, broadcasting, etc.) or of a practical nature (daily contact with the representatives of public authority), the process involved is necessarily a very slow one. In the beginning only a small part of the population—*viz.* the representatives of urban communities dealing with the authorities—will learn the language of the latter. In time, the number of the learners will increase for economic and social reasons, but for a very long time these, too, will remain bilingual, using the new language in a more or less correct form in public relations, but continuing to speak the old one in their family and social life, so that both languages will still be kept apart on two different levels. Only by the time when the new language will begin to replace the old, will the contact between the two become more intimate in the sense that the former will be interpenetrated by elements of the latter. There is no reason to assume a different development for the countries East of the Mediterranean, where Aramaic was spoken in certain districts up to the 17th century and is still spoken in three villages of the Anti-Lebanon[5].

It therefore seems indicated to assume that phonetic changes in the Arabic dialects of the region are possibly or probably due to Aramaic influence only with regard to periods for which they are attested directly (by contemporary literary sources) or indirectly — insofar as they serve to explain phenomena occuring at some later period.

[4] The earliest reference to the influence of Aramaic on the phonetics of East Arabic dialects (elision of short vowels and anaptyxis of the resulting consonant clusters) is found, as far as I know, in an article by 'Aqlīmīs Dā'ūd, Syrian Catholic Bishop of Damascus, in *al-Mašriq* I (1898): 844. Cf. also Bergsträsser (1915): Introduction *in fine*. For Aramaic influence on the lexicology of Eastern Arabic dialects of the bibliography see Reich: 5 fn. 3 and statements by Barthélémy, *passim*.

[5] Reich: 5; cf. the amusing example of 'mixed speech' on p. 30.

0.3. It goes without saying that, precisely owing to the gradual manner in which most, if not all, of the phonetic changes referred to in this paper have occurred, it is extremely difficult to determine the exact time in which a given change has taken place. Any division of the material into 'stages' must therefore be somewhat arbitrary and at any rate approximate. A special difficulty with regard to the dating of phonetical and/or phonological changes in Arabic dialects in general is caused by the constant—and in recent times increasing—borrowing of lexemes from the literary language. The average educated or half-educated Arab is practically bilingual in this respect—but in this respect only. The author of this paper cannot concur in the opinion expressed by Smeaton[6] that this bilingualism extends in most cases to the domain of phonetics and exerts far-reaching influence on the phonology of the dialects; rather are the phonemes of most learned word heard for the first time automatically 'translated' into those of the dialect (*e.g.* -/a(h)/ in most cases > /e/, /q/ > /ʔ/, etc.). This state of affairs has probably existed for centuries and accounts for many difficulties with regard to the dating of phonetic changes, especially in connection with loanwords. For if a given old phoneme has undergone a change in some dialect and this change can be observed in borrowings from a foreign language, it can never be argued that the phonetic development has taken place later than the borrowing, as some of these loanwords (particularly Turkish words) have first been taken over into the written chancellery language, and then treated by the dialect exactly like elements of literary Arabic, *i.e.* automatically adjusted to the phonemic pattern of that dialect.

0.4. From what has been said in 0.2 it follows that most of the changes noted for one particular urban dialect are common to all sedentary dialects of the region comprising Syria, the Lebanon and Palestine (except the rural and semi-rural dialects of Central and Southern Palestine, which present a more or less marked admixture of Beduin dialects). These dialects can be grouped conveniently under the name of East Mediterranean Arabic (EMA) to distinguish them from Eastern Arabic proper; the latter group is represented by the dialects of the Taurus region and Upper Mesopotamia (now within the limits of Turkey) as well as those of Middle and Lower Mesopotamia (now within the limits of Iraq), which have preserved a number of older phonological features

[6] Smeaton: 358-361.

owing to the fact that they have escaped the levelling influence of the great centers situated on or near the Eastern shore of the Mediterranean.

As for Egyptian Arabic, it has many features in common with EMA—again owing to the mutual influence just mentioned; on the other hand, it shows marked divergencies from it in phonetics/phonology and grammar, but the factors contributing to these are outside the scope of the present investigation.

A special problem in this respect is presented by the dialect of Jerusalem, which includes two sub-dialects, that of the Moslems and that of the Christians. The latter reflects two opposite tendencies present in the social and cultural life of any minority not confined to the borders of one particular country: the centripetal tendency of forming a distinctive group with its own patterns or behavior, speech, etc.; and the centrifugal tendency of intermarrying with members of the same community in other districts and countries. With regard to the Christians of Jerusalem the second tendency seems to be a stronger one, as the overwhelming majority of their families descend in the maternal line from Galilean or Lebanese women. The differences between the two sub-dialects, which lie chiefly in the domain of phonetics and phonology, are to a large degree neutralized in the speech on the younger Moslem generation, owing to the influence of women-teachers in kindergartens and elementary schools, who until recently have been almost exclusively Christians.

0.5. In the following an attempt will be made to trace the phonetic changes in EMA in general, and in the Jerusalem dialect in particular, through consecutive stages. The latter dialect will be dealt with chiefly in Section 5, where general methods of phonemic analysis will be suggested; the details will be left to a future systematic phonological description of this dialect.

1. *Stage 1. The language of the Arab conquerors (7th-8th centuries).*

1.1 *Consonants.* The nature of the consonantal phonemes of Arabic at that stage, their classes and the oppositions between the latter have been conclusively described by Cantineau (1946). The following remarks may, however, be added to his analysis:

The term 'emphasis' that has hitherto generally been applied by Western scholars only to 'velarized' buccal consonants including uvular /q/ (which is found in peninsular dialects in Southern Yemen), is used in this paper as the common term of all consonants

produced with a contraction of the entire pharyngeal tract and the glottis, whether accompanied by velarization or not. It is therefore taken to include fricatives of both the pharyngeal (or rather the faucal) order /ḥ/,/ʿ/ and the uvular order /x/, /γ/[7]. With regard to /ḥ/ and /ʿ/, their 'emphasis' has been remarked by the Arab grammarians—explicitly for /ʿ/ and implicitly for /ḥ/;[8] besides, their 'emphasizing' influence on plain consonants, *e.g.* in Hebrew (Bibl. /t-ʿ-0/ > Mishn. /ṭ-ʿ-0/ 'err') is a long-established fact. This being the case, the emphasis of /x/ and /γ/—also noted, though not as clearly, by the Arabs themselves[9]—must equally be assumed, for only thus can the mergers of /x/ and /γ/ with /ḥ/ and /ʿ/ in Aramaic and Canaanitic be explained. As to /ʾ/, it is difficult to accept Martinet's position[9a] that it was 'emphatic', being glottalized; quite apart from the fact that the identification of 'emphasis' with glottalization in Common Semitic remains a moot point,[9b] the Arab phoneticians expressly warn against an 'emphatization' of /ʾ/, which would otherwise become /ʿ/.

With regard to the action of the vocal chords in the production of the 'emphatics', it must be remarked that, while /š/, /x/ and /ḥ/ were breathed, the rest—including the interdental fricative /ð/ (Cantineau's *ḏ*) and the lateral[10] fricative /ƛ/ (Cantineau's *ḏ̣*) —were voiced, though to be sure the plosives /ḍ/ and /ġ/ (corresponding respectively to Common Semitic /ṭ/ and /ḳ/) had—if the actualizations in the modern peninsular dialects can be taken as witnesses—full voice only between voiced sounds and were partly voiced in other positions. In any case, the voiced nature of /ġ/ < *ḳ in Arabic would then explain the shift of *g to [ɟ] and later to /ǰ/[11], which was parelelled in some dialects by the change of

[7] Cf. now Jakobson 1957: 106 f., though his thesis that /x/ and /γ/ are the 'emphatic' correspondents of /š/ and /ž/ (!) can hardly be accepted, if only for the reason that the consonants of both groups occur in contact with each other in Arabic root-morphemes.

[8] Bravmann: 39f., 41f.

[9] Schaade: 15; Bravmann: 30, 44.

[9a] Martinet 1953: 70.

[9b] Cf. the author's Observations sur les phonèmes de l'hébreu massorétique et traditionnel, BSL 50 (1954): 234ff.

[10] Schaade: 18.

[11] This has been suggested to me by Professor Martinet. To be sure /g/ has been retained in South Western Yemenitic, where ḳ has become /q/ (as in Aramaic and Canaanitic); but the developments in South Eastern Yemenitic—*ḳ to /Ǧ/ (and [-γ-]) and *g to /ɟ/~/ǰ/ (cf. C. Landberg, Étude sur les dialectes de l'Arabie méridionale, I vol., Leide 1901: XXI and 485; 32; and the author's own observations) still remain to be explained.

/k/ to [c] and later to /č/. The palatalization of **g* did not for the time being represent a change of order, though it was destined to do so in the future.

The oppositions within the several orders were, then, plain vs. 'emphatic', and within these series, breathed vs. unbreathed.[11a] The whole system, which was strongly asymmetrical, is shown in the following table:

Plain	breathed	/f/	/t/	/θ/	/s/	/š/	/k/		/h/
	unbreathed	/b/	/d/	/ð/	/z/		/ɟ/		/ʔ/
'Emphatic'	breathed				/ṣ/			/x/	/ḥ/
	unbreathed		/ḍ/	/ð̣/	/ƛ̣/		/ġ/	/ɣ/	/ʿ/
Sonants		/m/	/n/	/r/	/l/				
Semivowels		/w/	/j/						

/ƛ̣/ already showed a tendency to merge with /ð̣/[12] owing to its isolated position within the system, while that of /š/ was somewhat stronger.

/ʔ/ showed a tendency towards weakening: it was frequently reduced to zero in word final and syllable final with concomitant lengthening of the preceding vowel *-a-*[13] (iʔC- >-i:C- was already accepted in the classical grammar), and occasionally also as the second element of a consonant cluster before -a-.[14] The actualization of the sequence -a:ʔi- as -a:ji- is equally of very old date, as well as that of -iʔa- and -uʔa- as -ija- and -uwa- respectively;[15] all these actualizations are reflected in the traditional consonantal spelling of Arabic.

Similarly, the semi-vowels /w/ and /j/ in syllable final had given way to length of a preceding homorganic vowel (/u/ and /i/ respectively).

Instances of consonant assimilation are the total assimilation of /ʔ/ and /w/ in syllable final to the infixed verbal morpheme *-t-*, and of *-l* of the definite article to a following dental, interdental and alveolar (both recognized by the classical grammar); to these must be added dialectal tendencies to assimilation in voice[16] and in 'emphatization' and 'de-emphatization'.[17]

[11a] Schaade: 20; Bravmann: 21ff.
[12] Fück: 89.
[13] Fück: 116.
[14] Schaade: 33; Fück: 147.
[15] Schaade: 34.
[16] Schaade: 46-48; Fück: 195 fn. 4.
[17] Fück: 90.

1.2. *Vowels.*

There were six vowel phonemes: /i/ /i:/ /u/ /u:/ /a/ /a:/, the latter two with numerous allophones ranging from [e:] to [ɔ:].[18]

The opposition of length[19] was significant, although long vowels were shortened before consonant clusters in external close juncture.

There was a dialectal tendency towards vowel assimilation of the type CaCi>CiCi when the intervening consonant was a pharyngeal or a laryngeal.[20]

The elision of final vowels in pausal forms was possibly due to the tendency to unvoice them in this position.[21]

1.3. There were the following *types of syllables:* CV, CV:, CVC, CV:C, CACC and CA:CC (the cluster in the latter type being geminate); the first two types occurred in any position, the others only in monosyllables and finally.

1.4, *Stress*, whether strongly or weakly marked, had no fixed place in the word.[22]

2. *Stage 2. (9th-10th centuries).*

2.1. *Consonants.*

It has been assumed (chiefly by German scholars and Cantineau)[23] that the merger of the interdental fricatives /θ/ and /ð/ with the dental plosives /t/ and /d/ respectively occurred under the influence of Aramaic. However, as Ferguson has justly pointed out,[24] the interdental phones have been preserved in Aramaic (and, one might add, in the Mesopotamian dialects of Eastern Arabic) and the

[18] Rabin (1951): 28f.

[19] It has already been pointed out (cf. Cantineau (1946): 130 f.) that the Arab grammarians considered vowel length as a sequence of a short vowel+homorganic semi-vowel. Blanc accordingly transcribes /i:/ and /u:/ as /iy/ and /uw/ (for which he is lauded by Birkeland (1954): 9), but retains /a:/. This inconsistency has been pointed out by Smeaton: 366. Phonetically, no semi-vowel is actualized here in any Arabic dialect except in peninsular and Beduin dialects (in pausal forms). Nor can such a transcription be justified on morphological grounds (cf. Smeaton 356f.). In morphophonological descriptions vowel-length can be treated as a linear phoneme /:/, to be replaced in certain derivational or inflexional patterns by /w/ or /j/, as the case may be.

[20] Schaade: 37.

[21] The actualization [Vh] and [V?] of final vowel signalled in modern peninsular dialects (cf. Goitein: XIII) must be considered as unvoicing.

[22] Birkeland (1954): 6,13; but cf. Weil.

[23] Cantineau BSL 40 (1938): 81 quoted by Fück: 91 fn. 24.

[24] Ferguson: 562.

change just mentioned is a common feature of many sedentary Arabic dialects. The early date of this change may be inferred from the statement by Saadya Gaon to the effect that the people of Palestine (in contrast to the Iraqians) failed to actualize the fricative allophones of the plosives in sandhi;[25] the context of this statement implies that he was referring to their Aramaic speech, in which case it might have been the peculiarity of a purely local dialect of that language.[26]

The 'emphatic' plosives /ḍ/ and /ġ/ became unvoiced in all positions; besides, the localization of the latter was shifted backwards, so that it became a uvular (cf. below).[27] The date of these changes can be inferred from the fact that Saadya Gaon makes no mention of a difference in the actualization of these two phonemes between Arabic and Hebrew. Cantineau[28] assumes Aramaic influence for the uvularization of /q/, but makes no mention of its unvoicing and of that of the dental 'emphatic' plosives.

Again Saadya Gaon bears witness to the fact that the voiced counterpart of /k/ had by now become a palato-alveolar affricate /ǰ/,[29] a fact that entailed a change of order, so that this phoneme now became the voiced counterpart of /š/.

Following the changes of /ḍ/ and /ġ/, /k/ became isolated within its order, but its position remained unweakened owing to the occurrence of numerous phonemes of the same series; cf. the following table:

Plain	breathed	/f/	/t/	/θ/	/s/	/š/	/k/		/h/
	unbreathed	/d/	/d/	/ð/	/z/	/ǰ/			/ʔ/
'Emphatic'	breathed				/ṣ/			/x/	/ḥ/
	unbreathed		/ṭ/	/ẓ/			/q/	/ɣ/	/ʕ/

Sonants and semi-vowels: as in Stage 1.

It is seen from this table that /q/ occupied a unique position within the system, being the only plosive which was in oppositon

[25] Saadia ben Joseph al-Fayyûmi, Commentaire sur le Sefer Yeṣira... publ. et trad. par Mayer Lambert (1891): 45.

[26] They have likewise been retained in the dialects of the Anti-Lebanon, cf. Reich: XII, *et pass.*

[27] To be sure there are exceptions: this phoneme is actualized as a post velar /k/ in the Anti-Lebanon (cf. Reich: XII) and in Nazareth (cf. Michel Ḥaddād in *Al-Mujtama*, Nazareth, April 1954: 14).

[28] Cantineau (1953): 104.

[29] Saadya Gaon, *op. cit.*,: 89f.

to a fricative within the same order and series. This fact lends support to the assumption of Aramaic influence, as Aramaic did not have uvular fricatives as *phonemes*, while the phones [x] and [ɣ] were probably not uvular.

/ʔ/ showed further evidence of weakening. In initial position it was sometimes replaced by /w/[30]; as to its replacement by /j/ in the same position (instances taken from modern EMA are: /jasi:r/ 'prisoner of war' for /ʔasi:r/, /jansu:n/ for /ʔa:nisu:n/ 'anis-seed'), it may also be very old; in any case it is certainly due to Aramaic influence, as is witnessed by dialects of that language. The elision of ʔi- in a closed syllable in sandhi, when followed by a syllable in V:, is witnessed for the 9th century by the transcription *ʔabū sḥāq* for *ʔabū ʔisḥāq*[31] 'father of I'.

In final position it was now commonly reduced to zero. It is further not impossible that the assimilation of /ʔ/ as the second element of a final consonant cluster to a preceding semi-vowel (e.g. /ðaww/ < /ðawʔ/ 'light' (subst.)', /fajj/ < /fajʔ/ 'shade') is equally of old date.

At the same stage /h/ of the feminine morpheme was eliminated in word final following a long vowel.[33]

The change of the sonant /l/ to /n/ in word final in partial assimilation to a sonantic semi-plosive (e.g. /ʔisma:ʔi:n/ for /ʔisma:ʔi:l/, /ʔazra:ʔi:n/ for /ʔazra:ʔi:l/) is witnessed for the 10th century.[34]

If indeed Arabic /ʔaj/ 'that is' is based on Middle Persian /e:/ 'this'[35], the sequence -aj- in monosyllabic root-morphemes began to be actualized as [e:] around the 9th century. This would probably entail the parallel actualization of -aw- as [o:].

Consonant assimilation in 'emphatization' and 'de-emphatization made further progress at that stage.[36]

2.2. *Vowels.*

The main development at that stage was the defi nite elision of short vowels in word final (and in stem final, cf. below).

[30] Fück: 116.
[31] Fück: 81.
[33] Fück: 116.
[33] Birkeland (1952): 12 f.
[34] Fück: 150.
[35] Fück: 71.
[36] Fück: 90.

Long vowels could be shortened in certain positions, e.g. *ʿajšah* for /ʿa:jišah) f. name.[37]

The assimilation of -a- to -i- of the following syllable is now witnessed also for cases in which the intervening consonant is phonetically a semi-vowel; cf. the significant spelling *sitti* 'my lady' (for sajjidati, through sijjid(a)ti:), attested for the 9th century in Iraq;[39] it is more than probable that the masculine form of this noun /sajjidi:/ has undergone a parallel change to /si:di:/ (through /sijj(i)di:/).

On the other hand, the change of -i- to -a- in forms of the pattern Cicci:C (*e.g.* /biṭṭi:x/>/baṭṭi:x/ 'watermelon', /xinzi:r/>/xanzi:r/ 'pig' etc.) cannot be regarded as dissimilation, but rather as a *restitutio in integrum* of the Common Semitic pattern (in contrast to the assimilated form of Classical Arabic); this change certainly occurred under Aramaic influence, unless the non-assimilated form already existed in one of the old Arabian dialects.

The spread of the dialectal change of -a- to -i- in the pre-formative of the aorist (/jaktub/>jiktub/ 'he writes, will write') equally belongs to that period.[38]

2.3. The *types of syllables* remained on the whole the same as in Stage I, with the exception that type CA could no longer occur finally, and that a new type—CCV:C—was introduced (initially in monosyllables) in sandhi.

2.4. *Stress conditions* remained the same as for Stage I.

3. *Stage 3. (11th-15th centuries).*

3.1. *Consonants.*

It is to be assumed that at that stage the voiced interdental emphatic' fricative /ð̣/ became a dental plosive /ḍ/, following the definite change of the plain interdental fricatives to dental plosives (cf. 2.1). That this change could not have occurred earlier follows from a statement by Saadya Gaon[40] to the effect that /d/ of the Hebrew word (ʔappadno/ 'his palace' was actualized in his days like the Arabic 'emphatic' fricative.

Following the change of /ð̣/ to /ḍ/ this phoneme moved into the dental order and a correlation was established between it and /ṭ/,

[37] Fück: 65.
[38] Feghali (1919): 102, fn. 2.
[39] Fück: 59.
[40] Saadya Gaon, *op. cit.*

which in its turn was a result of the unvoicing of old /ḍ/. This led to a change in the nature of the opposition within the 'emphatic' series, which now became voiceless *vs.* voiced; with regard to plain consonants the opposition breathed *vs.* unbreathed remained unchanged in view of the correlation between /h/ and /ʔ/.

A further significant change took place at that stage, *viz.* the merger of /q/ with /ʔ/, which is witnessed for the contemporary pronunciation of Syriac in Palestine by Barhebraeus.[41] This development, which was doubtless preceded by a 'de-emphatization' of /q/ (for otherwise the latter would have merged with /ḳ/), possibly represented at first a local peculiarity, but in the course of time spread to other urban and later also to most rural communities of the Eastern Mediterranean region.[42] It was favored by the progressing weakening of /ʔ/; following the merger this phoneme could now in all positions, including word final and syllable final.

The resulting consonantal system of EMA at that stage presents itself as follows:

Plain	breathed	/f/	/t/	/š/	/s/	/k/	/h/
	unbreathed	/b/	/d/	/z/	/ǰ/		/ʔ/
Emphatic'	voiceless		/ṭ/	/ṣ/		/x/	/ḥ/
	voiced		/ḍ/			/ɣ/	/ħ/

Sonants and semi-vowels: as in the preceding stages.

The initial sequence ʔi- in closed syllables, which had begun to be elided in external close junction in the preceding stage (cf. 2.1), was now elided in other positions also; this follows from the fact that after the introduction of fixed wordstress (see below 3.4) verbal forms of the themes ʔinCaCaC(a), ʔiCCaCC(a), ʔiCtaCaC(a), and ʔistaCCaC(a) were never stressed on the first syllable, but received penultimate stress and had consequently changed to the patterns nCaCaC, CtaCaC, CCaCC and staCCaC.

That /ʔ/ was elided in external close juncture is witnessed in Mozaraic poems, where *wʔajn (wa-ʔajn)* 'where' is treated like a monosyllable.[43]

[41] Barhebraeus, Livre des Splendeurs (Syriac text) ed. A. Moberg, 1922: 205.

[42] Cf. Bergsträsser (1915), map 4, but see above, note 27). The /q/ of the Moslems of Nablus (as against Bergsträsser) may be due to a conscious opposition to the /ʔ/ of the Samaritans, which varies freely with /q/ even in their reading of religious texts in Hebrew and Aramaic.

[43] *E.g.* in the verses in the Diwan of Yehuda ha-Levi ed. Brody, I Berlin 1901, pp. 183 and 185.

/h/ of the suffixed morphemes began to be eliminated in word final even after short vowels. While accepting Birkeland's opinion that the elision of /h/ in this position must have occurred later than its elision after a long vowel,[44] I cannot concur in his view that it must have taken place later than the shortening of final long vowels[45] as 'no short vowels were permitted at an earlier stage'.[46] From the fact that (inflectional) final short vowels were elided at Stage 2 (cf. § 2.2) it does not necessarily follow that final short vowels of a different morphological nature could not occur in Arabic dialects until after the shortening of originally long vowels, which certainly took place at a very late stage (cf. below, § 5.20). On the contrary, certain changes in vowels can be explained only by the elision of final suffixed /h/ precisely at Stage 3 (cf. below, §§ 3.2 and 4.2). And lastly, the regular Arabic-Hebrew rhymes *-ah/-a* and *-uh/u* (written *ū/u* in the mixeds poems of the 12th-13th centuries and possibly even earlier,[47] as well as the utter lack of *-h* in medieval Spanish transcriptions of Arabic words,[48] serve to witness the elision of final /h/ in Spanish Arabic, where, to be sure, it was bound with (phonetic?) length of the preceding vowel; a parallel development can be argued *a fortiori* for EMA, *i. e.* for dialects strongly influenced by Aramaic, where it was surely of very old date.

The above-mentioned mixed poems also contain rhymes like *-ajnu/-enu*,[49] which testifies for a further development of /aj/ to [e:] (and probably also of /aw/ to [o:]).

Further cases of assimilation of consonants in voice and 'emphatization' and 'de-emphatization' probably occurred at that stage as a parallel to those found in the pronunciation of Syriac.[50]

[44] Birkeland (1952): 12f.

[45] *Ibid.*: 18.

[46] *Ibid.*: 22.

[47] Diwan of Mose Ibn Ezra ed. Brody, Jerusalem 1935, pp. 240f., 266; Diwan of Yehuda ha-Levi, ed. Brody I, Berlin 1901, pp. 92, 144, 158, 187, 196, 201; H. Brody, Die Gürtelgedichte des Todros Abulafia, *Mitteil. d. Forschungsinst.* f. Hebr. Dichtung, I, Berlin 1933, pp. 27, 44, 53, 69, 77,f., 95; Diwan d. Abraham Ibn Ezra, ed. Rosin, Breslau 1885-88, p. 83, ed. Eger; Berlin 1886, p. 110; H. Brody, *Yedi'ot hammaḳon l-ḥeqer haššira ha'ibril* VI, Jerusalem 1946, pp. 70-77; H. Schirmann, *ibid.* II (1936), pp. 137, 140f.; Ibn Quzmān, ed. D. Ginzburg, St. Petersburg, 1896.

[48] Cf. Steiger, *passim*.

[49] Schirmann, *loc. cit.*: 47.

[50] A. Merx, *Historia artis grammaticae apud Syros*, Leipzig 1889, 53f., 121f., enlarged by Barhebraeus: 205f.

3.2. *Vowels.*

/a:/ in word final tended to be shortened and treated as the feminine morpheme; this development, which had begun in isolated cases at the end of Stage 2,[51] presents an additional testimony to the elision of final /h/ after short vowels (cf. § 3.1).

Following the introduction of fixed word stress (cf. below § 3.5) /i/ and /u/ in (unstressed) closed final syllables came to be actualized as [e] and [o] respectively. Previous to that, /a/ in the suffix *-ah* had changed after front consonants in EMA—and possibly in Eastern Arabic in general—to /i/ (*e.g.* /sanah/>/sanih/ 'year'), which now became [e] (*e.g.* [saneh]). The actualization of -iC as -eC in this position is confirmed for Spanish Arabic by the Arabic-Hebrew rhymes *ʾalbāriḥ* ([ʾalbe:reaḥ]?)—*jareaḥ* and *is*—*eṣ*;[52] at any rate its occurrence in EMA is shown by the subsequent development: after the elision of /h/ in word final, [e] and [o] could occupy this position (cf. below § 4.2) and possibly receive phonemic status (for a detailed analysis see below § 5.22): /sane/ 'year', /ša:fo/ (<[ša:foh]</ša:fuh/) 'he saw him'. Since in most Lebanese dialects /i/ and /u/ remained unchanged in this case, although they became /e/ and /o/ in closed syllables in general, it must be assumed that -/h/ had been elided there at an earlier stage.

This development, in conjunction of the actualization of /aj/ as [e:] and of /aw/ [o:], gave rise to new vowel phonemes, *viz.* /e/?, /e:/, /o/? and /o:/.[53]

As to the actualization of /a:/ as [ɔ:] after back consonants in North Lebanese dialects[54] it must be very old, as it preceded the merger of /q/ with /ʔ/ (e. g. [ɔ:l] (</qa:l/), 'he said', [ʔɔ:m] (</qa:m/) 'he rose' etc.). With regard to the actualization of /a:/ in the vicinity of front consonants as [e:] in the same dialects and as a result of *Umlaut* before a syllable containing /i/ both in these dialects and in those of North Syria,[55] no date can be fixed at present.

[51] Fück: 145f.

[52] Yehuda ha-Levi: 185; Todros Abulafia ed. D. Yellin, Jerusalem 1936, II 2, p. 73.

[53] Cantineau (1956): 119-121 considered e and o as realizations of *ay* and *aw* (in contrast to the transcription adopted in his *Manuel*). It is difficult to see why /e:/ and /o:/ should be more 'alien and confusing' in EAM (cf. Smeaton: 367) than in other Semitic languages, where they are partly of the same origin.

[54] Bergsträsser (1915) map. 5.

[55] Bergsträsser (1915) map 6.

3.3. *The syllable types* remained on the whole the same as for stage 2; the type CCa:C became more common, and the types CCA and CCaCC were introduced; the former initially and the latter in monosyllabic verbal forms.

3.4. *Stress.* The most important change that occurred at that stage was the introduction of fixed word stress, for which the following patterns can be established: (a) the stress fell on the final syllable if the latter contained 4 morae, *i. e.* was of the type CV:C or CACC; (e.g. sa'la:m 'peace', ka'tabt 'wrote, thou (m.) wrotest'); (b) failing that, it fell on the syllable nearest to the final, if it contained 3 morae, *i.e.* was of the type CV: or CVC (*e.g.* ma'da: ris 'schools', ši'ribna: (<*/šaribna:/*) 'we drank'); (c) failing that it fell on the initial syllable (e.g. '*ʔ*akalu: 'they ate', štaɣalat 'she worked'. To be sure, there were exceptions to the rule of pattern (c) in some of the EMA dialects; thus, it the aorist of verbs, the preterite of which began with a consonant cluster (of the type /staɣal/ mentioned above), the stress fell on the penultimate (*e.g.* niš'tiɣil 'we (will) work') in analogy to the basic form of the preterite.[56]

The dating of the introduction of fixed word stress is made possible directly by the changes of vowels conditioned by it. (cf. § 3.2) and indirectly by its occurrence in identical patterns in Spanish Arabic, as is shown by the final stanzas *(xarǰāt)* of the girdle-poems *(muwaššaḥāt)*, where tonic meters were definitely beginning to replace quantitative metres[57] (the development of stress patterns in North African dialects of Arabic has been entirely different).

[56] Bergsträsser (1915) map .

[57] *E. g.* two stanzas in the Diwan of Yehuda ha-Levi

(a) in Spanish Arabic (ed. Brody I, 1 p., 185 according to Ms. Oxford 1971, 136, vo.):

yā ḥamām ʔalḥallāq	'O inconstant dove,
yā ḥanīnī	o my love!
wajn ɣibt ʔalbāreaḥ	where wert thou yestereen
lam lajīnī	(that) thou didst not come to me?

Note the anapaestic (tonic) metre of the stanza in contrast to the metre of the (Hebrew) poem: - ∪ - | - - - | - ∪ - -

(b) in Mozarabic (*op. cit.* I, I, p. 172):

jā rabb como vivreyo	'O Lord! How shall I live
cun eslo ʔalḥallāq	with this inconsistant one,
ya man qabl ʔan yusallim	who, ere he greets (me),
yuhaddid bilǰirāq?	threatens (me) with parting?

Note the iambic (tonic) metre of the stanza in contrast to the metre of the (Hebrew) poem - - - - - - - - - - - - - -

In absence of a general investigation into the incidence of stress in language and its socio-psychological conditioning, it is at present impossible to decide what factors caused the introduction of fixed word stress in sedentary Arabic dialects in general and EMA in particular. In any case it is difficult to accept the opinion expressed by Rabin[58] that it possibly occurred under Aramaic influence. Although we have as yet no means of determining the exact stress pattern (or patterns) of medieval Aramaic, it is safe to assume that is was mechanical, *i.e.* either penultimate (as in most Neo-Aramaic dialects) or morphologically conditioned (as in older Aramaic); in neither case could it have influenced the stress patterns of EMA, which are conditioned by prosodic features.

The fixed word stress, though predictable and therefore non-phonological, was to have far-reaching influence on the vowel system and syllable structure of EMA, of which there are no indications at Stage 3.

4. *Stage 4. (16th-18th centuries).*

4.1. *Consonants.*

The main changes in the consonantal system of EMA at that stage were caused by the introduction of Turkish as the official language of the region. That language, on its part, contained numerous Arabic elements borrowed mainly through the medium of Persian.

In those elements, the old interdental phonemes /θ/, /ð/ and /δ̣/ were actualized respectively as [s], [z] and [ẓ] (a velarized [z]). The same actualizations were introduced into EMA in words pertainning to the learned or official style of speech, as well as in words used by, or in contact with representatives of the government.[59] This has given rise to such lexical alternants and variants as /ḥadi:s/ 'oral tradition (in Moslem canonic law)' *vs.* /ḥadi:t/ 'conservation' (both <*/hadi:θ/), /zakar/ 'he mentioned' *vs.* /dakar/ 'male' (both <*/ðakar/), /ɣali:ẓ/ ∾ /ɣali:ḍ/ 'coarse'. It must be noted that Christian dialects show on the whole a preference for plosives,[60] *e.g.* /matalan/ (<*/maθalan/) 'for instance', where the Moslem dialects have /s/.

[58] Rabin (1955):37.

[59] Feghali: 37f. has also noticed this fact; he assumes, however, that the Turks actualized /ð̣/ as plain [z] and that the Arabs emphasized this sound in order to make it conform to the phonemic pattern of their language.

[60] Feghali: 49 records the same with regard to the speech of the old Lebanese peasants.

In this way, a new phoneme /ẓ/ was introduced into EMA as a voiced counterpart of /ṣ/, This development was facilitated by the fact that the phone /ẓ/ already existed in EMA as a positional variant of /ṣ/ in contact with a voiced or partly voiced consonant, *e.g.* [ẓɣi:r] (/ṣɣi:r/ <*/ṣaɣi:r/ 'small, little', [bazqa] (/baṣqa/, originally /baṣġah/) '(action of) spitting', [lazqa] (/laṣqa/, originally /laṣġah/) 'sticking-plaster', (from the latter two the root-morphemes subsequently changed to /b-ẓ-ʔ/ and /l-ẓ-ʔ/).

As for the 'emphatic' dentals in general, their chief feature became during that stage—possibly strenghtened by the influence of Turkish—velarization rather than pharyngealization.[61] Prior to this development occured the change of /t/ to /ṭ/ and /d/ to /ḍ/ in the numerals 11-12 in partial assimilation to /ċ/, e.g. /ḥḍaċš/ '11', /ṭnaċš/ '12' etc. Parallel to this, /x/ and /ɣ/ were tending to be de-emphaticized.

/q/ had now definitely merged with /ʔ/. The /ʔ/ in borrowings from Turkish, e.g. /ʔu:ša:n/ 'title-deed' *(<kušan)*, /ʔabaḍa:ji:/ 'bully' (*kabadajī*, lit. 'stout uncle'), /ʔarnabi:ṭ) 'cauliflower' *(<karnabīt)* is in itself not conclusive with regard to the dating of the change, as it may constitute a case of 'phonetic translation' (cf. above, § 0.2). It is significant that in later immediate borrowings postvelar Turkish /k/ was rendered by /k/, e.g. /karako:z/ *(<karaçöz)* 'puppet show', /karako:n/ *(<karakol)* 'police station'; this, however, may also be due to a more fronted actualization of this phoneme in later Turkish.

The consonantal system of EMA in general at that stage may thus be represented as follows:

Plain	breathed	/f/	/t/	/s/	/š/	/k/	/x/	/h/
	unbreathed	/b/	/d/	/z/	/ǰ/		/ɣ/	/ʔ/
'Emphatic	unvoiced'		/ṭ/	/ṣ/				/ḥ/
	voiced		/ḍ/	/ẓ/				/ʕ/

Sonants and semi-vowels: as in the previous stages.

Owing to stress /ʔ/ was now introduced word initially in forms where Old Arabic had it only in utterance initial preceding a prosthetic vowel, *e.g.* /na:di: ʔibni:/ (</na:di-bni:/) 'call (f.) my son', /ha:da: ʔismi:/ (</ha:ðа-smi:/) 'this is my name', /ʔu:mi: ʔiftahi:/ (</ġu:mi-ftahi:/) 'get up (f., and) open (the door)!'. (This development has not taken place in the Syrian and Lebanese dialects with regard to the imperative, where the

[61] Feghali: 41, 56f., 67f.; Blanc: 53.

consonant cluster has been retained.) A similar development seems to have brought about the change of /kam/ 'how many, how much' to /ʔakam/ 'how many'.

On the other hand, the elision of initial /ʔ/ together with its vowel (in open unstressed syllables) or alone (in closed unstressed syllables) now progressed further; thus /bi-ʔide:h/ 'with his hands' became /b-i:de:h/, /w-ʔiza ka:n/—'and if' >/wiza ka:n/ and /ʔiza ka:n/ >/zaka:n/ 'if', /ja:ʔabu:ʿali:/ 'O father of Ali' >/ja:-bu:ʿali/, /ja: ʔumm ʔahmad/ 'O mother of Ahmed' >/ja-mm ʔahmad/, to say nothing of composite words, such as /ja:ba:/ (</ja: ʔaba:h) 'daddy', /jaxi:/ (</ja: ʔaxi:/) 'O brother' etc., /šusmo/ (</šu: ʔismo/) 'what's-his-name'. The same took place after the elision of /i/ in the preformative of the aorist, *e.g.* /biʔasʔal/>/basʔal/ '(shall) ask'.

A further testimony for the elision of /ʔ/ after external close juncture is presented by the retention of the *-t* suffix in numerals preceding nouns in plural with the preformative *ʔa-* (and only in this case), *e.g.* /tala:tatʔawla:d/ (</θala:θat ʔawla:d/), >/talatt wla:d/ 'three children', /ʔarbaʿatʿa:la:f/ >/ʔarbaʿt ala:f/ '4000', /xamsat ʔašhur/>/xamst ušhur/ 'five months' (cf. also § 5.3).

The elision of /ʔ/ following external open juncture, in conjunction with the tendency to change /a/ to /i/ in closed syllables and the elision of short /i/ in the following open (unstressed) syllable, has eventually led to the neutralization of old ʔaCCa:C and CiCa:C plural patterns of the noun even in initial position, both becoming CCa:C, e.g. /ʔasna:n/ 'teeth'> /sna:n/ and /bila:d/ 'towns, country, countries'> /bla:d/. The same is of course true with regard to the definite article (in initial position): /ʔal/-> /il-/>/l-/.

The now fully developed word stress probably played its part also in the secondary reduplication of the final consonants in nominal stems of the pattern CVC and C(V)CVC except when the final consonant was a sonant; thus, /ʔab/ 'father' >/ʔabb/ (free form), /ʔax/ 'brother (free form)' >/ʔaxx/, /dam/ 'blood' >/damm/ (but not in the derived form /damawi:/ 'bloody'), /θum/ (dial.) 'mouth' >/tumm/, /ǰihah/ 'direction, part'> /ǰihha/, /luɣah/ 'language' >/luɣɣa/, */mija/(</miʔa/) >/mijje/ '100', */muruwa/ (</muruʔa/) >/mruwwe/ 'manliness, chivalrousness'; in contrast to /sanah/ 'year' >/sane/, /marah/ 'woman, wife' >/mara/.

/h/ had definitely become elided in word final after short vowels, as is shown by the fact that final /a/ and /e/ in borrowings from Turkish are treated phonologically and morphologically as a

feminine suffix, *e.g.* /ʔišle/ (<*kïsla*) 'barracks', /baʔla:we/ (<*baklava*) 'a species of cake', /buʔǰe/ (<*bokča*) 'bundle'. It was likewise elided—probably at the same stage—in composite words such as /šu:/ (</ʔajj šajj hu:/) 'what', /mu:š/, later /muš/ owing to proclisis (</ma:hu:š/), and /mi:š/, later /miš/ (</ma:hi:š/) 'is not' (m. and f. respectively), /ʔanu:/ (f. /ʔani:/, pl. /ʔanumme/) 'which' (</ʔajn hu:/ etc.).

The partial assimilation of /-l/>/-n/ (cf. above § 3.2.) was still operative, as is shown in Turkish loanwords as /karako:n/ (<*karakol*) 'police station' and /burdʔa:n/ (<*portokal*) 'oranges'; the latter instance also shows assimilation in voice of /t/ to /r/, with which it has come in contact owing to the elision of the intervening short vowel (see the following §).

It is possible that the following cases of assimilation of consonants also took place at that period: *-ṣf*>*-ṣṣ* in /niṣf/> /nuṣf/ (cf. below § 4.2) >/nuṣṣ/ 'half'; *-hš*>*-šš* in /ma: fi:hš/ 'there is nothing in it'>/ma:fihš/ (with shortening of the long vowel preceding the consonant cluster (cf. § 4.2)>/ma:fišš/ (and later /ma-fišš/) 'there is (are) not' and /ma:ṣale:hš/ 'there is nothing to it' >/ma:ṣalešš/ (and later /ma-ṣalešš/) 'it doesn't matter'; a variant of the later is /maṣle:š/. Assimilation in external close (later internal) juncture is represented by *-rl-*>*-ll-* in /ʔawfar li:/>/ʔawfalli:/ (later /ʔawfalli/) 'it is more economical for me (and thus throughout the paradigm)', /ṣa:r li:/>/ṣarli:/>/ṣalli:/ (later /ṣalli/) (šahre:n ho:n) 'I have been (here for two months)' (and thus throughout the paradigm).

4.2. *Vowels.* At this stage the incidence of stress began to manifest itself with regard to vowels in syllables not affected by it.

The short high vowels /i/ and /u/ came to be elided in open unstressed syllables, *e.g.* /biˈla:d/>/bla:d/ 'towns, country, countries, /riˈkibna:/ (</rakibna:/)>/rkibna:/ 'we rode', /ṣa:liˈḥi:n/>/sa:lḥin/ 'pious (pl.)', /kuˈru:m/>/kru:m/ 'vineyards', /ˈja:kulu:/</ja?kulu:/) >/ja:klu:/ '(that) they eat', /muṣalliˈmi:n/>/mṣallmi:n/ 'teachers, masters'. This fact too, is partly supported by the treatment of unstressed /i/ and /u/ in Turkish loanwords, e.g. /ʔaškara (xabar)/ (<T-Pers. *ašicāre*), 'overtly', /brinǰi/ 'first-class' (<*birinǰi* 'first'), /burdʔa:n/ (<*portokal*) 'oranges', /kundra/ (<T.-Gr. *kundura*) 'shoes', /ʔma:š/ (<*kumač*) 'woven fabric', /kre:k/ (<*cürec*) 'shovel'.

/i/ and /u/ were retained in unstressed open syllables only in personal names, as in /ba:kize/ (T.-Pers. *pākize*) f. n., /fuʔa:d/,

/muni:r/ (both m. n.), /wida:d/ (mostly f.n.), as well as in words borrowed from the literary language, as /mudi:r/ 'director, manager', /za:kira/ (<*/ðа:kirah/) '(faculty of) memory', /l?aγnija/ 'the rich (pl.)' (the genuine dialectal form is /γana:ja:/, later /γana:ja/).

As to the low short vowel /a/ when unstressed, it received different treatment according to the various dialects. In the dialect of Jerusalem, with which this paper is chiefly concerned, it has been retained in this position to a greater degree than in other EMA dialects. It has been elided in pretonic open syllables (a) following its assimilation to /i:/ of the following syllable, unless protected by a preceding laryngeal/pharyngeal or 'emphatic'[62] *e.g.* /bcḥi:d/ (<*/biḥi:d/</baḥi:d) 'distant', /sli:me/ (<*/sili:me/</sali:mah/) 'safe and sound (f.); don't worry (about a spell of fever, a wound etc.)!', in contrast to ?ari:b (</qari:b/ 'near', /ṭawi:l/ 'long, tall', /ṣaḥi:h/ 'true' (but. cf. /ṣḥi:ḥ/ 'whole'); (b) in verbal preformatives (except after /?/) *e.g.* /tku:ni:/ (</taku:ni/) '(that)thou (f.) be', /jcḥidd/ (</jaḥidd/) '(that) he count', /nru:ḥ/ (</naru:h/) '(that) we go', /tfaḍḍal/ (</tafaḍḍal/) 'please (come in, take a seat, help yourself, etc.)', /tša:raku:/ (</taša:raku:/) 'they became partners' (in the last two forms the elision of /a/ in this position has extended throughout the paradigms of all tenses and moods). In posttonic syllables /a/ has been elided medially if this syllable or the next was followed by the *t*-morpheme, *e.g.* /katbat/ (</katabat/) 'she wrote', /badlti:/ (</badlati:/) 'my suit (of clothes)', /xubzto/ (</xubzato/ </xubzatuh/) 'his piece of bread', /madrstak/ (</madrasatak/) 'thy (m.) school'; for the latter form cf. below, § 5.2. In all other cases /a/ has been retained in unstressed open syllables in Jerusalem dialect.

It is difficult to fix a date for the neutralization of /i/ and /u/ into a single phoneme with various actualizations according to the consonantic environment, which has generally taken place in the Syrian and Lebanese dialects.[63] In the Palestinian dialects there is hesitation between /i/ and /u/ in closed syllables, *e.g.* /baḥibb/∿/baḥubb/ 'I like, love', /mislim/ (</muslim/) 'Moslem', /mirkin/ (</murkin/) 'relying (part.)', /mittif?i:n/ (<*/muttif?i:n/ </muttafiqi:n/) 'agreeing (part. pl.)', in contrast to /mu?dir/

[62] As Blanc: 32 has justly pointed out.

[63] Bergsträsser (1924): 8ff.; Feghali: 102-104, 113; Cantineau (1953): 15.

(</muqdir/), 'being able to afford', /muržde/ (</muršida/ </muršidah/) f.n., /muknse/ (<*/miknse/</makniseh/) 'broom (implement)'.

Equally difficult to date is the assimilation of vowels to the vowel of the following syllables in the Jerusalem dialect (a) of /i/ to a following /a/ or /a:/. *e.g.* /šatranǰ/ (</šitranǰ/) 'chess', /maʔla:/ (</maqla:/) 'frying-pan' and (b) of /a/ to a following /u/ in the plural pattern ʔ*a*CC*u*C, *e.g.* /ʔunfus/ (</ʔanfus/) 'souls (persons)', /ʔušhur/<(/ʔašhur/) 'months', /ʔurγfe/ (<*/ṣarγufe/ </ʔarγifah/) 'loaves'. As the last example shows, this process must have taken place later than the beginning of the variation between /i/ and /u/.

The followings types of vowel assimilation are in all probability late: /a:/>/e:/ in the sequence -*a*:C*i*-, the only instance being /mba:riḥ/ (</lba:riḥ/)>/mbe:riḥ/ 'yesterday'; the form /se:ṣa/ ∼/sa:ṣa/ 'hour, clock, watch', which has hitherto remained unexplained, may be due to analogy to the same type of assimilation in the dual /sa:ṣte:n/ [se:ṣte:n], later /saṣte:n/ (with no variant [seṣte:n]); /a/>/i/ in the feminine suffix of -*i*-verbs, *e.g.* /ṣimlat/ (*/ṣimilat/</ṣamilat/)>/ṣimlit/ 'she did, made, (coll.) said', /mišjat/ (<*mišijat/</mašijat/)>/mišjit/ 'she walked'. As for the Christian variants of the aorists /jo:kul/∼/ja:kul/ (</jaʔkul/) etc. '(that) he, etc., eat' and /jo:xud/∼/ja:xud/ (</jaṣxud/ etc. '(that) he, etc. take', I am at present at a loss to explain them; it is barely possible that they are due to assimilation of /a:/ to /u/ of the following syllable; at any rate it does not seem probable that they present an instance of the actualization of /a:/ as [ɔ:] or [o:], in view of the lack of conditioning in both cases (cf. § 3.2).

4.2.1. *Long vowels* in open syllables still remained unaffected by (lack of) stress, as is witnessed by the Turkish loanwords /ʔu:ša:n/ (<*kušan)* 'title-deed', /ba:bu:ǰ/ (<*pabuč*) 'slippers' /ba:bu:r/ (<*vabor*) 'steamer, Primus cooker', /ša:ku:š/ (<*čecič*) 'hammer', /ṭa:bu:r/ (<*ṭabur*) 'battalion' (with the respective plurals /ʔawa:ši:n/, /bawa:bi:ǰ/, /bawa:bi:r/, /šawa:ki:š/, /ṭawa:-bi:r/). It is, however, possible that long vowels became shortened before consonant clusters already at that stage (*e.g.* /l-ḥaǰǰṣali:/ (<ʔal-ḥa:ǰǰ) 'the Mecca-pilgrim Ali' (and hence the f. /ḥaǰǰe/), /šabb/ (</ša:bb/) 'young man', /ʔal-li/ (</qa:l-li:) 'he told me', /ṣallo/ (</ṣa:r-lo/) (šahre:n ho:n/) 'he has been (here for two years)', /ma:kanš/ (/ma: ka:n-š/) 'he (there) was not',

/la: tǰibš/ (<*/la: tǰi:b-š/) 'do not bring', /ma: bafutš/ (<*/ma: bafu:t-š/) 'I do (shall) not go in'.

The phonemic status of the long vowels /e:/ and /o:/ (cf. 3.2) was now strenghtened owing to their occurrence in numerous loanwords from Turkish and Italian, *e.g.* /baze:lja/ (<T.-It. *bezelja*) 'peas', /ro:sto:/ (<It. *rosto*) 'roast (joint of meat)', /šo:rba/ (<T. *čorba*, possibly <Arab. /š-r-b/) 'drink') 'soup'.

4.3. The *types of syllables* were now as follows: CV, CVC CVCC and V: in all positions, CCV: only initially, CCVCC in monosyllabics; CCV:C both initially and in monosyllabics.

5. *Stage 5. (18th?-20th centuries).*

5.1. *Consonants.* It was probably at that stage that /ǰ/ became /ž/ in most EMA dialects, except that of Aleppo[64] (which was strongly influenced by Turkish) and that of the older generation of Moslems in Jerusalem where the actualization [ǰ] still persists word initially. On the other hand, it has received in the dialect of Aleppo a voiceless counterpart in /č/ (originating mostly in borrowings from Turkish, but found also in autochtonous dialect words).

The fact that /ž/ now became the voiced counterpart of /š/ is substantiated by the total assimilation of the definite article to it, *e.g.* /ž-žabal/ 'the mountain' (whereas /l/ is never assimilated to /ǰ/).

/g/ was now introduced into EMA in loanwords from other languages and dialects, *e.g.* /gadaɛ/ (from Egyptian Arabic) 'brave fellow', /mangal/ (<T. *mangal*) 'brazier', /šangal/ (<T. *čengel*) 'hook'. This phoneme is as yet marginal, as no minimal pairs can be found illustrating an opposition between it and /k/, but both consonants occur in analogous environment;[65] cf. /mangal/ 'brazier', /ʔankar/ 'he denied'.

Following the de-pharyngealization of the old dental and dental-alveolar 'emphatics' the main classes of opposition for the buccal consonants now became 'plain' vs. 'velarized', 'emphatics' in the proper sense of the term being retained only for the extra-buccal order:

[64] Barthelemy, s. v. et *passim*.
[65] Cf. cf. Pike: 75 a-b.

Plain	voiceless	/f/	/t/	/s/	/k/	/x/		plain breathed	/h/
	voiced	/b/	/d/	/z/	/g/?	/ɣ/		unbreathed	/ʔ/
velarized	voiceless		/ṭ/	/ṣ/				emphatic breathed	/ḥ/
	voiced		/ḍ/	/ẓ/				unbreathed	/ʕ/

(for the majority of EMA dialects)

Plain	voiceless	/f/	/t/	/s/	/š/	/č/	/k/	/x/	plain breathed	/h/
	voiced	/b/	/d/	/z/		/ǰ/	/g/	/ɣ/	unbreathed	/ʔ/
velarized	voiceless		/ṭ/	/ṣ/					emphatic breathed	/ḥ/
	voiced		/ḍ/	/ẓ/					unbreathed	/ʕ/

(for the Aleppo dialect)

Sonants and semi-vowels: as in the previous stages.

The elision of /ʔ/ together with the following /a/ in the verbal form /ʔaCCaC/ following external close juncture takes place in the single instance of /maḥla:/ 'how sweet, pretty he (it) is!' (and thus throughout the paradigm), probably owing to the high frequency of its use (in contrast to, *e.g.* /maʔazka:/ 'how tasty!'). Besides, /ʔ/ (<q/) tends to be elided optionally after /i/ finally and /i/ medially, *e.g.* [bila:wi] (<bjila:wiq) 'he apes, will ape', [dʔi:a] (<dʔi:ʔa/) 'minute (unit of time)'. The latter case presents a difficult problem, as the final syllable begins with a vowel contrary to usual pattern.

/h/ now became elided in the personal objective suffix of the 3rd p. sing. m. (in word final) after a long vowel in EMA in general, as well as in morpheme initial of the personal (possessive and objective) suffixes *-ha*[66] 'her' and *-hum* 'their, them' (except in the urban dialects of Central and Southern Palestine).

The semi-vowels /w/ and /j/ have undergone further reduction, especially as members of consonant clusters. Although no exact date can be fixed for those changes, they must be comparatively late, *i.e.* have occurred after the definite elision of short vowels in (unstressed open syllables). Thus, as first elements of initial two-consonant clusters following external open juncture /w/ and /j/ are actualized as short homorganic vowels—the first invariably and the second optionally, *e.g.* [ʊ'rɑ:ḥ] 'and he went' [ʊ'we:n] 'and where', [ʊ'la:do] 'his children, [ɛ'ku:n]∾[jku:n] '(that) he be'; these actualizations are non-phonemic, as no syllable can begin with a vowel; hence /wra:h/, /wwe:n/, /wla:d/, /jku:n/. As the second element of such clusters /w/ is elided as a result of

[66] It is not clear from Sibawajhi *Kitāb, ṣilmi l'ṣulūm*, Bulāq, 1316 H: II, 262, whether /h/ in the sequence *-aha:* (where it is described as *xafijjah* 'covert') was merely weakened or actually elided; in any case the weakening did not take place in *-uha:*.

dissimulation after a labial: /biddi/ 'I want' (and thus throughout the paradigm) (</bwiddi/) '(it is) in my wish', /majje/ 'water' (</mwajje/ dim. of /ma:j/, with retention of the allophone [ɑ]), in contrast to /šwajje/ 'a little', dim. of older /šajj/<(šaj:ʔ); /j/ is elided in this position in the sequence /bj/—in the speech of Christians and young people, e.g. /bisʔal/∼/bjisʔal/ 'he asks, will ask', /bu:ṣal/∼/bjiwṣal/ 'he reaches, will reach', /bo:kul/∼ /bja:kul/ 'he eats, will eat', /bo:xud/∼/bja:xud/ 'he takes, will take.'

As the first element of a medial two-consonant cluster in the sequence -*iw*C-, /w/ is replaced—again in the speech of Christians and the young people—by length of the preceding vowel, which becomes homorganic, *e.g.* /tu:lad/∼/tiwlad/ '(that) she give birth'; /ʔu:ɋa/∼/ʔiwɋa/ 'beware'; /j/ in the sequence -*ij*C- is invariably replaced by length of the preceding vowel, *i.e.* /ji:bas/ (</j-b-s/) '(that) it become hard'; it goes without saying that in unstressed syllables this length becomes lost again, *e.g.* /biku:n/ /(<bijku:n)/ 'he is (habitually), will be'.

However, /w/ and /j/ have been preserved in this position in the bases after the pattern *ʔa*CC*a*C, *e.g.* /ʔawžaɋ/ 'more painful; he (it) caused pain', /ʔajbas/ 'harder'; moreover, new sequences of the type -*aw*- and -*aj*- have emerged from old -*a:wi*- and -*a:ji*- through elision of /i/ and —later— shortening of /a:/ in unstressed open syllables, *e.g.* /nawlu:ni!/ (</na:wilu:ni:!/) 'pass (pl.) me!', /sajri:hum/ (</sa:jiri:hum/) 'take (sg. f.) them by kind words!'.

As second members of final two-consonant clusters, /w/ and /j/ have become homorganic vowels, *e.g.* /ḥilu/ (</ḥilw/<ḥulw/) 'sweet, pretty, nice', /ḥaki/ (</ḥakj/) 'talk'; these actualizations conform to the regular occurrence of /u/ and /i/ in word final, but morphonological rules will have to be set up for their relation to the allomorphs with /w/ and /j/ (before vocalic suffixes).

As central members of medial three-consonant clusters in internal juncture, /w/ and /j/ are actualized as homorganic short vowels, *e.g.* [mašitak] 'your (sg. m.) (way of) walking', ['fʌrʊto] 'his fur'; these actualizations are non-phonemic, as /i/ and /u/ do occur in medial open syllables (except in a few 'learned' words, cf. § 4.2); hence /mašjtak/, /farwto/; the actualization ['ʔɪxɪwti]∼ ['ʔɪxʊti] 'my brothers' is likewise non-phonemic, being at variance with the stress pattern; hence /ʔixwti/.

The reduction of geminate semi-vowel clusters preceding a consonant and following a homorganic vowel is very old (cf. above § 2.1). Modern examples are, for close juncture: /ʔu:wto/

(</ʔuwwto/</quwwatuh/) 'his strength', /ni:jti/ (</nijjti/ < /nijjati:/) 'my intention'; in external close juncture (in four-consonant clusters: /mi:t ʔirs/ '100 piasters' (no examples for /ww/ in this position); in five-consonant clusters /ʔu:wt rriżża:l/ 'the man's strength'.

As a single consonant /w/ has been retained finally after /a/ in /law/ 'if (rej.)', in contrast to /lo:ma/ 'if (not)'.

In stressed syllables *-a:ji-* has been regularly replaced by /e:/ in nouns, *e.g.* /ḥe:ṭ/ (</ḥa:jiṭ/</ḥa:ʔiṭ/) 'wall', /ċe:le/ (</ċa:jile/</ċa:ċilah/) 'family' (cf. § 2.1). /ri:ḥa/ (for /ra:jiḥa/< /ra:ċiḥa/) 'odor' seems to be an Aramaism.

The assimilation of consonants made further progress at this stage. While it is difficult to fix a date for the single case of /m/>/n/ in contact with /t/ (/ntala(:)/</mtala:/ 'it (he) was filled') and for the general case of /n/>/m/ in contact with /b/ (*e.g.* /žamb/ </ǰanb/ 'side', /sumbule/</sunbule/ 'ear (of corn)'), which affected the distribution of /n/ (cf. the respective plural forms /žawa:nib/ and /sana:bil/), other cases of consonant assimilation in contact could have occurred only later than the elision of short vowels. Thus /b/ of the indicative morpheme became /m/ before /n/ (the preformative of the 1st p. pl. of the aorist, *e.g.* /mna:kul/ (</bna:-kul/<*/bi-na:kul/) 'we (shall) eat', /mnisʔal/ (</bnisʔal/<*/binisʔ-al/ 'we (shall) ask'; this assimilation has been extended by analogy to verbal forms in which /b/ has been separated from a consonant cluster beginning with /n/ by a vowel, *e.g.* /minʔu:m/ (</binʔu:m/) 'we (shall) rise', /minċidd/ (</binċidd/) 'we (shall) count'.

Similar cases are the (progressive) change of /l/ >/n/ in contact with /m/ in the compound word /amnawwal/ </ʔa:mlċawwal/ 'last year'; the voicing of all dentals and dental-alveolars in contact with voiced consonants (*e.g.* /ẓda:rijje/</ṣida:rijje/) 'bib', /žda:d/ (</šda:d/</šida:d/) 'belt', /ražda/</rašda:/ f. n. and the change of the medio-passive indicator *t-* to /ṭ/ in contact with a velarized first radical, *e.g.* /ṭṭallaq/ (</taṭallaq/) 'he looked (at)', /ṭṣaffa/ (</taṣaffa:) 'it (the liquid) was strained'. Of special interest is the variation of plain dentals and dento-alveolars under the influence of, though not necessarily in contact with, the velarized variant of /r/ in the vicinity of back vowels and /a/[67], *e.g.* [ṭrɑ:b] (</t(u)ra:b/) 'earth, soil', [ḍa:ɾ] (</da:r/) 'dwelling', ['ḍʌfṭʌɾ] (</daftar/) 'ledger, note-book' (cf. the pl. [da'fä:tir]), [ḍʌɾb]

[67] Blanc: 52; Cantineau (1954): 13; Feghali: 69.

(</darb/) 'way', [ṭo:r] (</to:r/, old /θawr/) 'ox' (cf. the pl. [ti'ra:n]), ['ṣa:ṛa] (</sa:ra:/) 'Sarah'.[68]

On the other hand, velarized consonants tend to become plain in root-morphemes containing /j/, /i/ or /i:/, *e.g.* ['dịjji(ʔ)] (</ḍijji ʔ/, old /ḍajjiq/) 'narrow', [bi'si:r] (</biṣi:r/</bjiṣi:r/) 'it may be (done)', ['säddiʔ] (</ṣaddiʔ/, old /ṣaddiq/) 'believe (me)!', [falas'ti:n] (<falasṭi:n/) 'Palestine'.

For the present the velarized and de-velarized actualizations are more or less optional, so that the phonemic status of the resulting consonants cannot be fixed. As to future developments, it is more than probable that the influence of the literary language will eventually counteract this type of assimilation.

Mutual assimilation is represented by *žh>šš* in /wišš/ (</wižh/) 'face' and by the regular /ọh/>*ḥḥ*, *e.g.* /maḥḥa/ (</maọha:/) 'with her', /dallaḥḥa/ (</dallaọha:/), 'he petted her': partial assimilation by *lb>mb* in /mbe:riḥ/ (</lbe:riḥ/) 'yesterday', and *s>z* in /handaz/>/handas/ 'he arranged'; /xafi:r/ </ɣafi:r/ 'rural policeman'.

For *dissimilation* of consonants no date can be fixed at present. Instances are: /ʔ/>/h/, if the same or the following syllable contains /ʔ/, *e.g.* /huʔmur/ (</ʔuʔmur/) 'command!', /hibʔi/ (</ʔibʔi:/) 'remain' (imper. sg. f.), /haʔọud/ (</ʔaʔọudʔ/) 'shall I sit down?'; and /š/>/s/, if the following syllable begins with /ž/, *e.g.* /sažar/ (</šažar/) 'trees', /sažžaọ/ (<šažžaọ/), he encouraged'; this is undoubtedly an analogy to forms, in which /š/ and /ž/ were in contact, *e.g.* /ʔašža:r/ 'kinds of trees', /tašži:ọ/ 'encouragement'.

As to the *metathesis* /ž/-/z/ in /zawwaž/ (</žawwaz/) 'he married (tr.)' and derivates, it is probably due to the fact that the consonant demanding a stronger articularly effort /ž/ tended to occur in a stressed syllable.

Both kinds of combinatory changes in consonants listed above could only have occurred after /ǰ/ had definitely become /ž/.

5.2. *Vowels.*

5.20. In this section the features of vowel-length and stress

[68] Blanc: 52-73 considers practically all consonants as being susceptible of velarization *(taʃxi:m)*; it is difficult to see how extra-buccal consonants /h/, /ʔ/, /ḥ/, /ọ/ can be 'velarized' or, alternatively, how /ḥ/ and /ọ/ can be 'pharyngealized'. It seems that he identifies a backward actualization of /a/ with *taʃxi:m* of the preceding consonant, which is to some extent true for all languages.

are going to be discussed together, as the problems involved are interdependent. This discussion will be followed by an attempt to analyze the phonemic status of the different vowel-phones and of anaptyctic vowels, and, finally, by a discussion of the phonological word.

5.21 *Vowel-length and stress.* Long vowels in unstressed open syllables now negan to be shortened,[69] e.g. /sa:ṣadna:/ > /saṣadna:/ 'we helped', /fi: da:ri:/ > /fi da:ri/ 'in my dwelling'. The late date of this development can be inferred from the fact that it has not taken place in all sedentary EMA dialects. Thus, the speech of the older inhabitants of Nablus in Central Palestine is characterized as 'drawling' (/bimɣaṭu lkala:m/); for Central Lebanon it has been noted[70] that pretonic long vowels are retained (both in open and closed syllables); the originally long vowels in word final registered by Durand[71] for the dialect of Beyrouth are either '*de qualité indécise*' or else seem to represent non-phonemic pausal variants.

On the other hand, the elision of /-h/ after long vowels presents a crucial problem[72] which may be briefly resumed as follows: the final syllable of the type CV:h, which was stressed according to pattern (a) (§ 3.4), has retained its stress in spite of its change to type CV:, which was hitherto unstressed in word final; the two alternative solutions proposed are either that stress has now become phonological (in view of the supposed contrast /ʼḍarabu:/ 'they struck' vs. /ḍaraʼbu:/ 'they struck him') *or* that /h/ is still 'virtually' present in word final.

It is clear that this problem arises out of the difficulties inherent in an analysis of phonological features at the very moment of their change, as it were. On the other hand, however, an analysis of the present should not be hampered by adherence to any supposedly rigid structural pattern of the past, as this would be tantamount to denying the possibility of linguistic change in general. The development under discussion presents a typical case of a breach in a hitherto existing pattern and must be clearly recognized as such. To revert to the alternative solutions to our problem, the second solution must be rejected *a priori* on the grounds just mentioned; as for the first, it is based on the erroneous

[69] Cf. Ferguson: 567ff. as against Cantineau (1956): 119ff.
[70] Feghali: 93, 105, 109.
[71] Durand: 69-72;
[72] Birkeland (1952): 28-31; cf. fn. 69 of this paper).

assumption that long post-tonic final vowels exist in EMA: in actual fact, the opposition is not /'ḍarabu:/ vs. /ḍara'bu:/, but /ḍarabu/ vs. /ḍarabu:/. There seems, therefore, no way out of the difficulty other than to accept a change in the stress pattern (a) and accordingly to modify the rule by the insertion of (type CV:, or else, a new set of rules may be formulated, *viz.* (a) the stress falls on a syllable centering in V: or closed with -CC; if both types occur within the word, preference is given to the first; *e.g.* 'ʿa:wadu 'they did again', ṭarad'ti:ha 'you (sg. f.) drove her away', sala'mo:n 'salmon'; 'juktbu '(that) they write', ka'tabthum 'I (or you sg. m.) wrote them', ʔanda'laft 'sexton'; kušt'ba:n 'thimble', bašt'ri: 'I shall but it (him)'. (b) in the absence of syllable types described in (a), stress falls on a medial syllable closed with -C, *e.g.* ṣa'rafti 'you (sg. f.) spent (money)', mʿal'limti, 'my mistress', ku'tubhum 'their books'. (c) in the absence of syllable types described in (a) and (b), stress falls on the initial syllable, *e.g.* 'ṭabxat 'she cooked', 'mišju 'they walked', 'šabake 'net', 'malzame 'printer's form'. (It goes without saying that these rules presuppose a discussion of the permissible occurrences of syllable types and —by implication—of (phonological) syllable cuts).

A similar problem of a change in stress pattern is presented by the elision of /-h-/ in the possessive and objective pronominal suffixes denoting the 3d p. sg. f *(-ha)* and the 3d p. pl. *(-hum)* in most EMA dialects, where the stress in the forms so affected has not changed (*e.g.* ḍa'rabha>ḍa'raba 'he struck her', ḍa'rabhon > aḍ' rabon 'he struck them'—contrary to stress pattern (b)). Here again the exception to the existing stress-pattern must be clearly stated,[73] unless a syllable cut is present between the stem and the suffix, in which case the syllable cut is an actualization of /h/.[73a]

A further problem is presented by the seeming interdependence of stress and vowel length. In discussing it, Birkeland[74] states that the opposition long vs. short vowel is possible only 'in an accented, open (this refers only to Egyptian Arabic, I. G.), non-final syllable'. This formulation of the distribution of long vowels seems to imply *a priori* a phonological status for stress, which he seeks to prove. In reality—at least as far as EMA is

[73] Blanc: 29.

[73a] This problem has now been treated by Ferguson, 1957: 468-477, whose article came to the author's knowledge after the present paper had been submitted for publication.

[74] Birkeland (1952): 36-40.

concerned—stress is still dependent, as has been shown above, on the structure and position of syllables, and therefore predictable and non-phonological. This analysis cannot be invalidated by the fact that /:/ may be elided when, as a result of derivation or inflexion, another syllable receives stress, as may be illustrated by the following example: Given the form /sa:ḳad/ 'he helped' (where /:/ is conditioned morphemically); here the initial syllable was stressed according to pattern (b); if the subjective pronominal suffix indicating the 1st p. pl. *-na:* was added, the new form /sa:-ḳadna:/ became stressed on the penultimate (according to the same pattern); at a later stage the long vowels both in the initial and the ultimate syllables became short /saḳadna/; further, when the objective pronominal suffix denoting the 3d p. s. m. *-h* was added, the stress shifted (at a stage previous to the shortening of long unstressed vowels) to the final syllable (according to pattern (a): /ḳsaadna:h/; and, finally, it was retained there after the elision of -/h/, which constituted a modification of stress-pattern (a) /saḳadna:/. In a synchronical description (of Jerusalem Arabic, for instance) the results of these processes may be formulated by two morphophonological principles: (1) final vowels in primary and secondary bases and stems are lengthened in forms with added suffixes (including zero-suffix but excluding suffixes beginning with -jj-), *e.g.* /biddi/ 'I want'—/biddi:š/ 'I won't', /šuftu/ 'you (pl.) saw'—/šuftu:ha/ 'you saw her', /ʔakalto/ 'I (or you, sg. m.) ate it'—/ma-ʔakalto:š/ 'I (you) did not eat it', /maḳna/ 'meaning' -/maḳna:/ 'its meaning', /ḳakka/ 'Acre'—/ḳakka:wi/ 'of Acre (adj.); in contrast to /maṣri/ 'Egyptian'—f. /maṣrijje/. (2) long vowels in primary and secondary bases and stems are shortened in forms with added suffixes (a) if the new form contains V:, *e.g.* /ʔi:mi/ 'take away (sg. f.)!',—/ʔimi:/, 'take it (him) away!', /be:t/ 'house' -/bete:n/ 'two houses', /ḥe:fa/ 'Haifa'—/ḥefa:wi/ 'of Haifa' (adj.)', /šu:fu/ 'look (pl.)!'—/šufu:hum/ 'look at them!' /sa:ḳadu/ 'they helped'—/saḳadu:hum/ 'they helped them'; (b) if the following syllable of the new form is closed, *e.g.* /xu:ri/ 'parson'—/xurijje/ 'parson's wife', /jo:m/ 'day'—/jomitha/ 'at that time (on its day)', /ʔi:me/ 'value'—/ʔimithum/ 'their value', /he:lam/ 'he bluffed' /helamna/ 'we bluffed'; /so:gar/ 'he insured'—/sogartu/ 'you (pl.) insured', /ḥefa:wi/ 'of Haifa (adj.)—f. /ḥefawijje-/.

As to the lengthening of vowels as a result of stress, which doubtlessly plays an important part in Egyptian Arabic,[75] it

[75] Birkeland (1952): 40-42

occurs in EMA in monosyllable imperative forms, *viz.* (a) those of the root-classes C-V/w-C and C-V/j-C in all dialects (*e.g.* /ži:b/ 'bring!', /ʔu:m/ 'get up!', /na:m/ 'go to sleep!'; (b) those of the roots /ʔ-k-l/ 'eat' and /ʔ-x-d/ likewise in all dialects: 'take' (/ko:l/, xo:d/); and (c) in those of all verbs of the root-class C-C-C (1st theme) in Syrian and Lebanese dialects[76] (*e.g.* /nze:l/ 'go down!', /kto:b/ 'write!', /sma:ʿ/ 'listen!'). However, the lengthening of the basic vowel in case (a) is probably due to analogy with disyllabic forms (*e.g.* /ži:bi/ 'bring (sg. f.)!', /ʔu:mu/ 'get up! (pl.)', while in cases (b) and (c) it is due not so much to dynamic stress as to affective pitch accent (cf. below 5.22). The only other example of secondary vowel-length noted by me is /zi:na/ 'adultery' (</zina:/</zina:ʔ/); but this form, which is in all probability contaminated by /zi:ne/ 'décoration, illumination', is quite isolated; by its side occur forms like /riḍa/ '(Divine) grace, favour' (</riḍa:/</riðа:ʔ/) and /bina/ 'building' (</bina:/ </bina:ʔ/).

5.22. *Vowel phonemes of the Jerusalem dialect.*

The phonological status of the six historical vowel phonemes /i/, /i:/, /u/, /u:/, /a/, /a:/ can be illustrated by following oppositions between the several pairs: /diri/ 'he has learned (it has come to his knowledge)' vs. /di:ri/ 'pour in (imper. sg. f.)', /ʿišti/ 'you (sg. f.) lived' vs. /ʿi:šti/ 'my (way of) life' and—since it has been shown that stress is not relevant—/ʔiṭfi/ 'put out (imper. sg. f.)' vs. /iṭfi:/ 'put it out (sg. f.)'; /dura/ 'millet, maize' vs. /du:ra/ place name, /ṣurti/ (allomorph of /ṣirti/) 'you (sg. f.) became' vs. /ṣu:rti/ 'my picture', /sallamu/ 'they handed over' vs. /sallamu:/ 'they handed him (it) over'; /xalaf/ 'progenity' vs. /xa:laf/ 'he opposed', /mašje/ '(way of) walking' vs. /ma:šje/ 'walking (part. sg. f.)', /xarraṭ/ 'he turned (mech.)' vs. /xarra:ṭ/ 'turner'.

As to the comparatively late vowels /e:/ and /o:/, there can be no doubt with regard to their phonemic status,[76a] as they occur in the same (identical or analogous(environment as /i:/ and /u:/ respectively, *e.g.* [ro:ḥ] 'life' vs. (ru:ḥ] 'spirit', ['mo:zə] 'banana' vs. ['mu:nə] 'provisions' and [lbe:ḍ] 'the eggs' vs. [lbi:ḍ] 'the white ones', [me:l] 'inclination', vs. [mi:l] 'mile'. With regard to the short vowel phones [e] (and allophones) and [o] the position is more problematic. Historically, these vowels are an outcome

[76] Feghali: 137; Cantineau (1954): 22 (by implication).

[76a] Cf. now Ferguson (1957): 462-467.

of several developments: (a) actualizations of /i/ and /u/ in closed final syllables as [ə] and [o] respectively (cf. § 3.2);[77] (b) shortening of [e:] and [o:] in non-stressed syllables, *e.g.* [be′ru:t] 'Beyrouth', [jo′me:n] 'two days'; (c) in the case of [e] only, assimilation of /a/ to the following sequence—jje, *e.g.* /ɋa′lajje/ > [ɋa′lejje] 'on (upon, against)me'. These developments preclude a synchronical opposition /e:/ vs. /e/ and /o:/ vs. /o/. Moreover, [e] in open non-final syllables varies freely with [i] (though not *vice versa*), *e.g.* [sle′ma:n]∼[sli′ma:n], m. n., [ḥeṭa:n]∼[ḥiṭa:n] 'walls'. Nor can minimal pairs for the opposition [e] vs. [i] and [o] vs. [u] be found except in word final, *e.g.* /xubze/ 'piece of bread' vs. /xubzi/ 'my bread', /ša:fo/ 'he saw him (it)' vs. /ša:fu) 'they saw', and—in word medial—the probably single instance of /(ʔa:mat)/[ɋa′lejjə] '(she rose) against me' vs. /(ʔa:mat)/[ɋa′lijjə] 'Aliyye (rose)'. This of course involves the general problem of whether phonemic status can be posited for phones occurring contrastively in a single position, such as [o], or in very limited positions, such as [e] in our dialect. The alternative would be to assume neutral length for /e:/ and /o:/ and to state that these vowel-phonemes are actualized long except in specified positions. In the latter case it would follow that the dialect has eight vowel-phonemes, *viz.* /i/, /i:/, /u/, /u:/, /a/, /a:/, /e/ and /o/, the latter two with conditionned length.

5.3. *The phonemic status of anaptyctic vowels.* The elision of short vowels in open unstressed syllables has led to the formation of numerous consonant clusters of new types, some of which have subsequently been disjoined by anaptyctic vowels. The discussion of the cases in which the nature of the consonants requires anaptyxis and of the quality of the anaptyctic vowels belongs to a systematic description of the phonology of the dialect. If the following we shall deal only with the problem of the pho-i nemic status of such vowels.

5.3.1. *Two-consonant clusters.*

(1) *Initial clusters* following external open juncture may be actualized (a) as VCC—(*e.g.* [ɪk′ti:r] 'much', [ɪb′taɋraf] 'do you (sg. m.) know?', [il-′be:t] 'the house', [ɪdärs] 'the lesson'), or (b) as CVC—(*e.g.* [ʔə′ra:jə 'reading (vn)', [ɋa′mɪlna] 'we did'). No phonemic status can be posited for the anaptyctic vowels in

[77] Transcribed by most authors as *i* and *e*, by Feghali as *ẹ* and *ọ* and by Piamente by **e** and **o** except in the form mitil 'such as'.

either case, as (a) no phonological syllable in the dialect begins with a vowel; in cases like /talatt-ala:f/ '3000', /xamst-ušhur/ 'five months' the vowels /a/ and /u/ are bound to the preceding /t/ (cf. § 4.2); and (b) the phone [ə] does not occur preceding a medial syllable of more than two morae or a final syllable of more than three morae or containing a long vowel.

(2) *Medial clusters* present no problem.

(3) *Final clusters* preceding external open juncture and utterance final are frequently, especially in deliberate speech,—and in the case of a dental plosive followed by /t/ invariably—actualized as CVC (*e.g.* ['binət] 'girl, daughter', ['šʊɣʊol] 'work', [ɋʌ'xʌdət] 'I took, you (sg. m.) took', ['mutət] 'I (or you, sg. m.) (nearly) died'). There is no reason for not accepting the phonemic status of the anaptyctic vowels in such stylistic and pausal variants or allomorphs in most of these cases,[78] provided that /i/ is posited for [ə] and /u/ for /ʊ/ or [ɋ]: /bint/∼/binit/, /šuɣl/∼/šuɣul/, /mutit/; in cases like [ɋʌ'xʌdət], however, the phonemization of the sound in the final syllable would imply a change in the stress patterns (cf. rule (C), § 5.21), hence /ʔaxadt/.

5.32. *Three-consonant clusters.*

(1) *Initial clusters* of this nature following external open juncture are disjoined by [i] between the first consonants. *e.g.* [lik'ta:b] 'the book', [bis'se:f] 'with the sword', [win'käsar] 'and he (it) broke (intr.)'; in this case, the forms /li-/, /bi-/, and /wi-/ simply represent allomorphs of /l-/, /b-/, and /w-/ respectively before two-consonant clusters.

(2) *Medial clusters* in internal juncture are in certain cases disjoined by an anaptyctic vowel following the first consonant (*e.g.* ['ʔɪɪzli] 'go (or come) down (sg. f.)!', [jʊrʊbṭu] '(that) they tie', ['rʌʔʌbto] 'his neck'). Here the anaptyctic vowels cannot have phonemic status, as otherwise they would be stressed.[79] Geminate clusters followed by a homogenic consonant are disjoined by [i] between the second and the third element, e.g. ['massɪsi] 'let (sg. f.) me feel', ['mɪnnɪma] 'from us, than we (Chr.). This vowel cannot be regarded as phonemic as /i/ does not occur in this position, hence—/masssi/, /minnna/. (Incidentally the latter farm is irregular, as the final consonant in the stem *min-*

[78] Cf. Cantineau (1953): 17ff. 22.

[79] *Ibid.:* 121; Blanc: 26 assumes for this case an exception to he regular stress pattern.

is doubled only before pronominal suffixes of the singular).—The view[79a] that geminate clusters are simplified before a heterogenic consonant does not correspond to the authors own observations: thus, /šillto/ 'its skein' sounds very different from /šilto/ 'I (or you, sg. m.) took it away', and the same applies to /waṣṣlo/ 'take him (to a place' in contrast to /waṣlo/ 'his receipt'. Naturally a geminate cluster in syllable final—and in word final—can be actualized only as consonant length: [ˈšɪl:tǫ], [ˈwʌṣ:lǫ], like [ˈsǫl:ɪ] (/sɪl/) 'consumption', [ˈmʌṣ:] (/maṣṣ/) 'sucking'.

In external close juncture two cases are possible: (a) -CC-C-, where an anaptyctic vowel is inserted after the first consonant (*e.g.* [ˈšʊfətˈbe:t] 'I (or you, sg. m.) saw a house', [mɪtəlˈʔe:s] 'such as what?', [ˈxʌmʌsliˈra:t] 'five pounds (sterling)', (ˈsäbaç-daˈfä:tir] 'seven note-books'; in this case the anaptyctic vowel ɪs phonemic (cf. above § 5.31 (3) and a corresponding morpho-phonological or syntactophonological rule must be formulated.[80] (b) -C-CC-, where the anaptyctic vowel is inserted before the second consonant (*e.g.* [ḅki:t.ɪkˈti:r] 'I (or you sg. m.) wept much', [ɕašar.əʔˈru:š] 'ten piasters'; this case, in which the juncture (in contrast to cases like [ˈḅki:tḭ.kˈti:r] 'you (sg. f.) wept much' quite apart from the fact that in the latter case short /i/ is actualized as /ḭ/ as in absolute final)[81] is actualized as a clear syllable cut, is analogous to that described above (§ 5.31 (I), and the anaptyctic vowel is therefore non-phonemic. . A special case is represented by taxemes like (mnɪlˈbe:t] 'from (than) the house', which is historically the outcome of the phonetic sequence [ˈmɪn.ɪlbˈe:t], with elision of the syllable-cut between [n] and [ɪ], and consequently of the vowel in the initial (now open) unstressed syllable. In this case there is no way but to assume an allomorph /mni/ of /min/ before a form beginning with /l-/ or its alternants: /mni-lbe:t/, /mni-ssu:ʔ/ 'from the market'.

5.33. *Four-consonant clusters* occur only medially. In internal juncture such clusters (as well as the sequence -V:CCC-) are disjoined symmetrically by an anaptyctic vowel (*e.g.* [jɪšˈtɪγlu] '(that) they work', [xaˈlɪtha] 'her maternal aunt'); since this vowel forms the centre of a stressed syllable (which entails the shortening of a long vowel in the preceding syllable, as in the second

[79a] Blanc : 74 (guardedly), Ferguson (1757) : 464).
[80] Cantineau (1953): 34.
[81] As against Blanc: 24.

example), it must be considered as phonemic. In one type of cases, however (*viz.* in forms where e/a has been elided between a two-consonant cluster and a single consonant, and the t-morpheme followed by a vowel added to the resulting three-consonant cluster) the anaptyctic vowel is not stressed (*e.g.* [ˈmädrästo] 'his school', [ˈʔärmɪlto] 'his widow'); as the stress in these and similar forms is at variance with the regular pattern, the anaptyctic vowel cannot be phonemic; on the other hand it must be noted that in the speech of the younger generation syllables centering in such vowels tend to be stressed ([mädˈrästo], [ʔärˈmɪlto]) and therefore these vowels tend to be phonemized.

In external close juncture there are again two cases: (a) -CC CC (actualized, as- [CCV.CC]- (*e.g.* [ˈmʌrt.ɪšše:x] 'the sheik's wife', [ˈsäbc̣.əbˈju:t] 'seven houses (or single rooms)', [ˈxɪft.ɪkˈti:r] 'I was (or you sg. m. were) greatly afraid'); this case differs in no way from those described in §§ 5.32 (2) (b) and 5.31 (1); hence /mart š-še:x/, /sabc̣ bju:t/, /xift kti:r/. (b) -CCC—C-, where the three-consonant cluster at the end of the first member of the taxeme has its origin in the elision of the -e/a morpheme before the -t suffix followed by external close juncture; in this case, the cluster is disjoined by an anaptyctic vowel placed before the final consonant, and this vowel must be assigned (morpho)phonemic status, *e.g.* /birkit ma:j/ 'a pool of water', /ḍummit baṣal/ 'a bunch of onions'.

5.34. Five-consonant clusters occur only in external close juncture. They are of two types (a) -CCC—CC (where -CCC= -CCt), actualized as -[CICC.ˈCC]-, *e.g.* [ˈbädɪltɪžžu:x] 'the suit (made) of broad-cloth'; in this case both of the anaptyctic vowels are non-phonemic, the first because it is unstressed and the second for reasons stated above (§§ 5.32 and 5.31); hence /badlt žžu:x/ (b) -CCCC—C-, (where -CCCC=-CCCt, cf. § 5.33 (b)), actualized as [CCVCC.C]; *e.g.* [ˈmädräst.baˈna:t] 'girls' school'; here, again, the anaptyctic vowel is non-phonemic, being unstressed, hence /madrst bana:t/.

5.35. Six-consonant clusters likewise occur only in external close juncture and are of the type -CCCC—CC (where -CCC is -CCCt, as just described), actualized as [CCVCC.VCC], *e.g.* [ˈmädräst. ɪruhˈba:n] 'the convent (lit. 'monks') school'. In this case, too, both anaptyctic vowels are non-phonemic for the reasons stated in the preceding paragraph, hence /madrst rruhba:n/.

5.4. *The phonemic word and clisis.* In this paper bound morphemes are considered as part of the word, unless they have allomorphs (a) functioning as bases or (b) not forming a stress unit with the following form; in which cases they are considered as clitics. Examples: l-ʔi:d /lʔi:d/ 'the hand', š-šarr /ššarr/ 'the evil' ḍarab-na:-kum /ḍarabna:kum/ 'we struck you (pl.)', ḥake-ti:-lo /ḥaketi:lo/ 'you (sg. f.) told him', b-aqraf-š /baqrafš/ 'I don't know', mi-n-ʔu:l /minʔu:l/ 'we say'; in contrast to ma-ruḥ-t-iš /ma-ruḥtiš/ 'I (or you sg. m.) did not go' (cf. ma:-ni ra:jiḥ /ma:ni ra:jih/ 'I am not going'); fi-bla:d-na /fi-bla:dna/ 'in our country' (cf. fi:-hum /fi:hum/ 'in them'); mn-e:n /mn-e:n/ 'where from?' (cf. min-ni /minni/ 'from me'); b-ʔalam /b-ʔalam/ 'with a pen' (cf. bi-l-'ʔalam /bilʔalam/—contrary to the existing stress pattern—'with the pen'), la-ṣabi /la-ṣabi/ 'to a boy' (cf. la-ṣ-'ṣabi /la-ṣṣabi/ —contrary to the existing stress pattern—'to the boy'.

6. *Conclusions.*

After having followed the development of the sound system of the dialect from its origins in Middle Arabic to the present day, one may reach the following conclusions: the consonant pattern has become simplified and more symmetrical; the vowel system has been enriched by two phonemes, which, however, are not completely integrated into the relation length/shortness; the syllable structure has been enriched by many new types (specifically with initial consonant clusters); the word stress has become fixed in somewhat intricate patterns, though it has not become phonological.

Hebrew University, Jerusalem

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NATIVE REACTIONS TO TONES AND WORDS IN MAZATEC

SARAH C. GUDSCHINSKY

It is the purpose of this paper to present evidence that the observable reactions of a native speaker to his own language depend in part on the degree of his linguistic sophistication and in part on his transitory level of awareness, or focus. The paper thus addresses itself to problems of the psychological reality of linguistic units.[1]

The observations reported here are anecdotal rather than rigidly experimental in nature, but they are suggestive of rigorous experiments which might be designed. For our own purposes, they served as evidence of the validity of our linguistic analysis.[2] The observations were made on native speakers of Soyaltepec Mazatec[3] learning to read and write their own language, or trying to teach their language to foreigners.

[1] A recent discussion of these problems from the combined points of view of linguistics and psychology may be found in *Psycholinguistics: a Survey of Theory and Research Problems*, ed. Charles E. Osgood and Thomas A. Sebeok, Baltimore, 1954, pp. 50-73. The question has a considerable history in connection with the conception of the phoneme as a *Lautabsicht* in the writings of Baudoin De Courtenay and other European phonemicists. Edward Sapir's articles, "Sound Patterns in Language" and "The Psychological Reality of Phonemes," reprinted in *Selected Writings of Edward Sapir*, ed. David G. Mandelbaum, Los Angeles and Berkeley, 1951, pp. 33-60, also bear on this question. Cf. also the study by Charles C. Fries and Kenneth L. Pike (see footnote 2); Harry Hoijer, "Native Reaction As a Criterion in Linguistic Analysis," in the *Proceedings of the Eighth International Congress of Linguists*, Oslo, 1957, pp. 573-583; and Howard Maclay and Charles E. Osgoad, "Hesitation Phenomena in Spontaneous English Speech," to appear in *Word*, vol. 15, no. 1.

[2] See Charles C. Fries and Kenneth L. Pike, "Co-existent Phonemic Systems," *Language* 25.29-50 (1949): "Evidence for the phonemic structure of a language is of two kinds: (1) phonetic and distributional data of the traditional kind; and (2) the observable reactions of native speakers as they attempt to write or analyze their own language or to speak a foreign language" (p. 50). Further discussion of the relevance of native reaction to linguistic analysis may be found in Kenneth L. Pike, *Language in Relation to a Unified Theory of the Structure of Human Behavior*, Glendale, Calif., 1954, vol. I, pp. 25-27, 33-34, 92, 104, 155, 156.

[3] The Soyaltepec dialect of Mazatec was spoken in and near the village of San Miguel Soyaltepec, Oaxaca, Mexico. The observations recorded here were made in

The orthographic mastery of tone has been an obstacle in literacy efforts not only in Mazatec, but also in other tone languages. The technique which we used for teaching our Mazatec pupils to read and write the tones of their own language was essentially that which Pike has outlined for the linguistic analysis of tone.[4] The pedagogical technique consisted of three steps.

1. EAR TRAINING. In response to appropriate pictures presented by the teacher, the pupils pronounced contrasting words, e. g. *ni³ñu³* 'star' and *ni⁴ñu⁴* 'corn cake'. The teacher wrote these words beside the pictures, pointing out the correlation of the written numerals with the way the word "sings". The pupils were drilled in sight recognition and written reproduction of each pair of words until they could read them easily and write them from dictation. Words learned in this fashion were used as samples in the second step.

2. GROUPING OF WORDS INTO TONALLY UNIFORM GROUPS. The pupils were given a list of words and asked to decide about each word whether it was more "like *ni³ñu³*" or "like *ni⁴ñu⁴*" (e. g. *ča³ho³* 'powder', *ča⁴hnka⁴* 'liver', *su⁴se⁴* 'green', *ki³c?o³* 'he ground') The teacher wrote the words in the columns under the samples indicated by the pupils. Since the pupils made frequent errors in their tone classification, they were called upon to check the groups by pronouncing the words of each column. As Pike has pointed out, "the psychological pressure associated with hearing, one after another, a group of words supposedly uniform in pitch is so strong that deviations are much more readily apparent than when words of many different pitches are heard."[5]

3. ASSOCIATION OF TONE SYMBOLS WITH TONE PATTERNS. The teacher wrote the tone numerals on each word, pointing out that words in the same column all "sang alike" and therefore all required the same numerals. Then the pupils were drilled in the reading and writing of these words. Any lesson was considered as mastered when the pupils could read and write any word of a given pattern without reference to the sample. Changes of key of isolated words were controlled by placing words in frames.

The efficacy of the literacy techniques was tested by the use of tone stories. These stories were built around the tone pairs in such a way that many of the sentences were ambiguous except for tone; e. g., the sentences *č?a³ e² he³nta³ho⁴* 'his load tired him', and *č?a²?e⁴ he³nta³ho⁴* 'it tired his father-in-law'.

1. SOPHISTICATION. Our observations indicate that the training of the native speaker has a profound effect on the degree

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the spring of 1954 when Eunice V. Pike and I were on loan from the Summer Institute of Linguistics to the Instituto Nacional Indigenista of Mexico. I am indebted to Miss Pike especially for observations of the informant, and for her analysis of the tone system with its intricate patterns of tone sandhi: "Tonally Differentiated Allomorphs in Soyaltepec Mazatec," *International Journal of American Linguistics* 22.57-71 (1956). The language has four tones, numbered 1-4 *from high to low*. The numerals were used in the practical as well as in the technical orthography. The informant was Mr. Alberto Ruiz Clara of San Miguel Soyaltepec.

[4] Kenneth L. Pike, *Tone Languages: A Technique for Determining the Number and Type of Pitch Contrasts in a Language, with Studies in Tonemic Substitution and Fusion*, Ann Arbor, 1948, pp. 43-66.

[5] *Tone Languages*, pp. 53ff.

to which he is aware of the phonemes of his own language. We observed at least four levels of response which were clearly correlated with degree of specific training:

(a) in certain contexts, contrasting phonemes are sometimes judged to be the same, or repetitions of the same phoneme are judged to be different;

(b) a pair of words differing by a single phonemic opposition is judged "different" but the phonemic difference is incorrectly described;

(c) phonemes are abstracted and labeled correctly;

(d) allophones are correctly identified and labeled.

In the first stage of the literacy program, Mazatecs had to distinguish pairs of words differing as to tone. Where the pairs differed by tone only, the judgment "different" was always given. But when a pair of words differed segmentally as well, the pupils often judged tonally different words as tonally the same, or tonally identical words as tonally different (level *a*). One common distracting factor was a change in voice register between utterances. If a pupil pronounced *ni*^{3}*so*^{3} 'gourd', for example, in a lower register than *ni*^{4}*ñu*^{4} 'corncake', he judged them tonally the same. If, however, he pronounced *ni*^{3}*so*^{3} 'gourd' in one register, and then *ni*^{3}*ñu*^{3} 'star' in a different register, he judged them tonally different. Furthermore, words whose only tone difference occurred in an initial non-stressed syllable were frequently judged tonally same, e.g., *łu*^{4}*wa*^{21} 'parrot', and *ča*^{3}*kį*^{21} 'firewood'.

It is significant that as the pupils progressed in literacy, the accuracy of their tone judgments was improved as their linguistic sophistication developed. They were taught to use a preceding low frame as a point of reference, e.g., *su*^{4}*se*^{4} *łya*^{4}*wa*^{21} 'the parrot is green', and *su*^{4}*se*^{4} *ča*^{3}*kį*^{21} 'the firewood is green'. Pupils using such a frame identified tonal 'sames' and 'differents' with almost perfect accuracy, in sharp contrast with earlier failures.

A higher level of response was involved in the case of the informant who recognized a pair of words as different, but was unable to pinpoint the phonemes by which they differed (level *b*). The informant was an intelligent bilingual who was literate in Spanish and had no difficulty in reading and writing those phonemes of his language which were represented in writing by letters of the Spanish alphabet. In addition he had acquired some sophistication about tone in helping with our tone analysis; he listened for words which "sang alike" and also for pairs which

differed only by tone. When he pronounced words for us he frequently volunteered other words which were "just the same except for the tone." On one occasion, after pronouncing for us the word *tykwa*^{21} 'snail', he volunteered that another word—*tkwa*^{21} 'hail'—differed "only by tone." He manifested considerable surprise when he proved to himself, by humming the tone patterns, that the tone was the same. He continued to insist that the words were different, but made no judgment as to how they differed. We analyzed his behavior as follows: He reacted to all of the phonemes of his language, both segmental and tonal, in terms of same or different and he readily hypostasized those segmental phonemes which closely approximated the Spanish sounds he had been taught in school (e.g., he made statements such as "we pronounce this word with a *jota* [j]"), but he lumped non-Spanish oppositions, such as *t* vs. *ty*, into a single group he called "tone."

It was this same informant who provided a good example of recognition and labeling of allophones (level *d*). As a result of his Spanish training, he insisted on calling our attention to differences between [u] and [o], and between voiced and voiceless stops. Neither of these represent phonemic contrasts in Mazatec; [u, o] are in free variation, and voiced stops are the allophones of voiceless stops occurring after nasals.

The level at which all phonemes of the language could be consistently abstracted and labeled (level *c*) was demonstrated by the pupils of the reading class at the end of the course, when they could read and write proficiently.

It is interesting to note that the speakers of the Soyaltepec dialect of Mazatec do not focus on abstracted tone without specific teaching. This is in contrast to the situation in the Huatla dialect of the same language, where males use whistle speech as a form of communication, and therefore learn to abstract tone patterns from childhood.[6]

From these observations it may be concluded that the reactions of a native speaker, observed while reading and writing, are at a level of considerable linguistic sophistication. This implies that in devising rigorous procedures for discovering psycholinguistic units, great care must be excercised to control for the level of sophistication by making it uniform, or by considering its fluctuation when evaluating results.

[6] See George M. Cowan, "Mazateco Whistle Speech," *Language* 24.280-286 (1948).

2. Focus. Our observations led us to conclude that for the Soyaltepec Mazatecs the most usual unit for "encoding" is a word,[7] although focus can shift from words to morphemes, or to allologs (contextual variants of words), or to phonemes.

The evidence that the Soyaltepec Mazatec focuses on the word is the contrast between his reaction to internal sandhi (mechanical tone changes within the word)[8] and his reaction to external sandhi (mechanical tone changes between words). In general, the tone *within* words was reacted to as basic, regardless of the occurrence of basic or sandhi allomorphs of the morphemes comprising the words. These reactions were first observed in the informant in the course of the tone analysis while he sorted words or phrases into tonally uniform groups.[9] To check on our own hearing, we asked the informant, as he pronounced each such group, whether the members were tonally same or tonally different.

Isolated words which were morphemically simple, and which involved no sandhi when pronounced in isolation (in response to Spanish equivalents) were unhesitatingly grouped by the informant according to their phonemic tone pattern: he judged *ni*^{3}*ñu*^{3} 'star', *ni*^{3}*so*^{3} 'gourd', and *wha*^{3}*ya*^{3} 'make' as tonally alike, but different from the group *ni*^{4}*ñu*^{4} 'corncake', *su*^{4}*se*^{4} 'green', etc. Isolated words which were morphemically complex, including some in which the tone of the morphemes was affected by internal sandhi, he also grouped according to their phonemic tone patterns, regardless of the basic tone of the constituent morphemes. For example he grouped as tonally uniform a set of words of the pattern 3-2-4 including *či*^{3}*lu*$^{2?}$*e*^{4} 'his cat' (<*či*^{3}*lu*^{24} 'cat'), *na*^{3}*hmi*$^{2?}$*e*^{4} 'his thing' (<*na*^{3}*hmi*^{1} 'thing'), *ni*^{3}*tha*$^{2?}$*e*^{4} 'his hat' (<*ni*^{3}*tha*^{3} 'hat'), and *si*^{3}*nka*$^{2?}$*e*^{4} 'his shirt' (<*si*^{3}*nka*^{2} 'shirt').

This reaction appears routine—native speakers are expected to react to the phonemic patterns of their own language. Its true significance appears only by contrast with the informant's reaction to phrases involving external sandhi. He was asked to

[7] By "word" is meant what Eunice V. Pike calls "phonological word" (*op. cit.*, p. 57, fn. 3); i. e., a grammatical word with stress on the final syllable, or a grammatical word plus one or more enclitics.

[8] Tone sandhi between grammatical word and enclitic differs somewhat from sandhi within the grammatical word, and also from sandhi between words. By internal sandhi, I mean here both the sandhi within the grammatical word and the sandhi between grammatical word and enclitic. By external sandhi, I mean only the sandhi between words. For the details of all types of sandhi, see E. V. Pike, *op. cit.*

[9] K. L. Pike, *Tone Languages*, pp. 48-54.

pronounce pairs of phrases whose constituent words were represented by non-basic allologs because of external sandhi, and to judge whether or not they were tonally uniform. When he pronounced such a pair as *thị*^{3} *ni*^{1}*se*^{3} 'there are birds' (from *thị*^{21} 'there are' and *ni*^{4}*se*^{3} 'bird') and *thị*^{3} *ni*^{1}*se*^{3} 'there are officials' (from *thị*^{21} 'there are' and *ni*^{3}*se*^{3} 'officials') he insisted that they were tonally different, although his pronunciation of these two utterances was perceptually identical. It was only after he had repeated the phrases several times, and had hummed their tone patterns that he finally judged that in this particular context the forms *ni*^{4}*se*^{3} 'bird' and *ni*^{3}*se*^{3} 'official' were tonally the same. It is evident in this reaction that his morphemic analysis had interfered with his phonemic reaction.

Similar contrasting reactions were seen when we taught the informant to read tone. We experimented with tone written in each of two different ways: phonemically, exactly as it is pronounced, and morphotonemically, with each morpheme written in its basic form regardless of the sandhi that occurs. When the tone was written *phonemically*, the informant read individual *words* in isolation easily; he encountered no difficulty with such items as *č*$^{?}$*a*^{2}$^{?}$*e*^{4} 'his father-in-law' (*č*$^{?}$*a*^{1} 'father-in-law', $^{?}$*e*^{4} '*his*') and *thị*^{3}*re*^{24} 'he has' (*thị*^{21} 'there are', -*re*^{4} 'he'). But his reaction was sharply different when we asked him to read *phrases* which were written completely *phonemically* with all of the external sandhi symbolized: e.g., *thị*^{21} 'there are' plus *na*^{4}*ce*^{4} 'rabbit' written in its normal "perturbed" form *thị*^{3} *na*^{1}*ce*^{4} 'there are rabbits'. The informant read the phrase, at normal speech speed, as it was written, pronouncing *na*^{1}- at the high point of his normal speaking register, but he objected to the orthography. Putting his finger on the *na*^{1}- he said, "I am saying that with thick [i.e., low] voice; it ought to have a number 4 on it."

When the tone was written *morphotonemically*, the situation was reversed: We asked the informant to read *words* formed as compounds from two independent morphemes, with each morpheme written in its basic "non-perturbed" form regardless of which sandhi alternant occurred, e.g., we wrote *ya*^{21} 'wood' plus $^{?}$*wa*^{4} 'crooked' as **ya*^{21}$^{?}$*wa*^{4} 'hook' rather than in its perturbed form *ya*^{3}$^{?}$*wa*^{24} (the latter being the only actually occurring pronunciation). The informant had great difficulty in reading such words and sometimes had to try more than once before he was successful. We then asked the informant to read *phrases* which were written *morphotonemically*, with each word in its 'non-perturbed" form:

e.g., *thị*^{21} *ni*^{2}*ñu*^{4} 'there are corncakes' (always pronounced *thị*^{3} *ni*^{1}*ñu*^{4}). These phrases he read easily.[10]

We concluded from our observations that for the Mazatecs the word was a basic unit—a psychologically real unit. This conclusion led to an immediate practical application; we adopted a semi-phonemic notation for our practical orthography; we wrote each word as it occurs in isolation. Morphemes within each word were written in the particular sandhi variant that resulted from internal sandhi; but nowhere did we let external sandhi be represented in writing. This notation proved highly satisfactory in teaching pupils to read and write tone, and the conclusions we had drawn from observing the informant's reaction were reinforced by comparable incidents in the classroom.

It was also found that under special circumstances, the Mazatec's focus can shift away from the word level.

An incident illustrating focus on morphs as separate units has been described by Eunice V. Pike. "..*ču*^{49}*a*^{2}*ni*^{4} 'a red parrot' (*ču*^{4} 'animal', $^{?}$*a*^{3}*ni*^{1} 'red'). According to the perturbation rules, we would have expected this word to be **ču*^{49}*a*^{3}*ni*^{1}.... It is interesting to notice that our informant reacted to this as an exception in the following manner, 'if we pronounced this right it would be **ču*^{49}*a*^{3}*ni*^{1}, but nobody says it that way.' Or when I forgot about the fact that the word was an exception and pronounced it with **ču*^{49}*a*^{3}*ni*^{1}, he would say, 'That's right, but if you want to talk the way we do, you will say, *ču*^{49}*a*^{2}*ni*^{4}.'"[11]

Another shift in focus, away from the word was seen when a native speaker corrected a foreigner's pronunciation of the tone.

[10] It is up to future investigation to determine whether for the Mazatec the following two occurrences of *thị*^{3} (from *thị*^{21} 'there are') are psychologically equivalent: (1) in such contexts as *thị*^{3}*re*^{24} 'he has' where tone 3 is a part of the basic word pattern, and (2) in a context such as *thị*^{3} *na*^{1}*ce*^{4} 'there are rabbits' where tone 3 is a non-basic tone resulting from external sandhi, as a variant of the basic *thii*^{1}. Incidentally, a similar situation was described by Sapir (*op. cit.*, p. 40, fn. 2) in Sarcee, an Athabascan language with significant pitch differences. In that language, "There is a true middle tone and a pseudo-middle tone which results from the lowering of a high tone to the middle position because of certain mechanical rules of tone sandhi. I doubt very much if the intuitive psychology of these two middle tones is the same." The situation seems to be much more complicated than this in Soyaltepec Mazatec. There *does* seem to be a difference in the intuitive psychology of basic and perturbed tones. However, 'basic' appears to apply to the tone pattern of words in isolation whether or not there is internal sandhi, rather than strictly to the tones of non-perturbed morphemes.

[11] E. V. Pike, *op. cit*, p. 63, fn. 17.

If we said a phrase incorrectly—*thị*^{21} *ni*^{3}*so*^{3} instead of *thị*^{3} *ni*^{1}*so*^{3} 'there are gourds', for example—the informant immediately corrected us. From the data available it is uncertain, however, whether in such a case as this he was focusing specifically on the choice of phonemes, or on the choice of the allomorph (with the phoneme choice automatic).

This evidence for shifting focus implies a hierarchy (or perhaps rather hierarchies) of psychologically real linguistic units. It also suggests that in hearing and speaking there may be a complex shifting of focus within interlocking hierarchies of units. In devising tests for probing these hierarchies, a major problem is insuring the desired depth of focus in each test.

Summer Institute of Linguistics.

DISCUSSIONS

TRAVELS THROUGH SEMANTIC SPACE

URIEL WEINREICH

1. ATTENTION, LEXICOGRAPHERS. When a linguist turns to lexicography, he becomes a descriptive semanticist, whose purpose is to list the words of a language and to "state their meanings." But it is an open secret that the linguist-lexicographer is a scientist with a bad conscience, for he has not yet found a way of performing his task in a completely reasoned, verifiable manner: separating polysemy from homonymy, breaking down the meaning of a word into "sub-meanings," selecting the terms of the definition, supplying synonyms, etc. Discussions of these problems in recent years, both in introductions to dictionaries compiled by professional linguists and in the journals, have failed to produce a way out of the arbitrariness and subjectivity of lexicography.[1]

Therefore, when the linguist-as-lexicographer learns of a new "experimental semantics" (p. 226 of the book under review[2]) offering prospects of "a quantized Thesaurus" (p. 330), when he hears that in a neighboring discipline procedures have been devised which "objectify expressions of these subjective states [meanings]" (p. 125) and "completely eliminate the idiosyncrasies of the investigator in arriving at the final index of the meaning" (p. 126), he must sit up and take notice. Has the lexicographic millennium arrived?

[1] See, for example, Hans Kurath's introduction to the *Middle English Dictionary*, of which he is the editor (Ann Arbor, 1952). Cf. also Alf Sommerfelt, "Sémantique et lexicographie," *Norsk Tidsskrift for Sprogvidenskab* XVII (1956), 485-489; Finngeir Hiorth, "Arrangement of Meanings in Lexicography," *Lingua* IV (1955), 413-424; L. S. Kovtun, "On the Construction of a Lexicographic Article" (in Russian), in *Leksikografičeskij sbornik*, ed. O. S. Axmanova et al., I, Moscow, 1957, 68-97, and other articles in the same volume.

[2] Charles E. Osgood, George J. Suci, and Percy H. Tannenbaum: *The Measurement of Meaning*. 342 pp. Urbana: University of Illinois Press, 1957.

This new type of "psycholinguistic" research, entitled "the semantic differential" and heralding revolutionary things for lexicography, was first unveiled in 1952.[3] Charles E. Osgood, professor of psychology and director of the Institute of Communications Research at the University of Illinois, has been chiefly responsible for the new method. In the years since, he has, alone and in collaboration with numerous associates, conducted and published a stream of experiments to follow up his basic theories. The present book is a progress report on this entire family of studies, including materials from many unpublished dissertations by Osgood's students.

There are important aspects of this research which the linguist will not be qualified to judge, such as the details of the statistical design, the applications of the method to personality measurement, to psychotherapy, to advertising, or to mass communications. The integration of the experiments with Osgood's version of general learning theory also lies beyond the linguist's own field. This review thus does not attempt to describe the book in full, nor to give proportionate representation to its several parts. At the risk of passing over some of the most fascinating investigations recounted in the volume, the review concentrates on the book's relevance to descriptive semantics as the linguist-lexicographer knows it.

In §§ 2-6 of this discussion, the authors' own conception of "meaning" goes unquestioned, and their results are judged on the preliminary assumption that their use of the terme "semantic" corresponds to a linguist's expectations. In § 7, this assumption is itself subjected to criticism, resulting in a suggestion that what the "semantic differential" measures is not the "meaning," but chiefly the affect, or the "emotive influence," of words. In § 8, the consequences of this criticism are applied to problems of word combinations. Finally, § 9 broaches the question whether techniques similar to those used by Osgood, Suci and Tannenbaum might be utilized in a type of research whose linguistic-lexicographic validity would be less subject to doubt.

2. Is a Knife Humble or Proud? In the typical form of

[3] Charles E. Osgood, "The Nature and Measurement of Meaning," *Psychological Bulletin* XLIX (1952), 197-237. A briefer statement for linguists, by the same author, appeared in *Psycholinguistics* (Osgood and T. A. Sebeok, eds.), Baltimore, 1954, pp. 177-184. Cf. now also Rulon Wells' exegesis for linguists, "A Mathematical Approach to Meaning," *Cahiers F. de Saussure* XV (1957), 117-136.

a "semantic differential" study, a subject or group of subjects is presented with a number of pairs of antonymous adjectives, such as *good—bad*, *kind—cruel*, *wise—foolish*, *complex—simple*, *humble—proud*, etc. The scale between every pair of adjectives has seven places on it. The subject is then given a "concept," e.g. *knife*, and is instructed to place it in one of the seven positions on each scale. Thus, if he considers *knife* very good, he is to put a mark at the *good* end of the *good—bad* scale; if he considers it neither good nor bad, but neutral in this respect, he marks the middle position; and so on for each scale. For a "concept" thus rated on a set of scales, there emerges a ("semantic") profile formed by connecting the checked points of each scale. If the subject then rates another "concept" on the same set of scales, the two resulting profiles can be compared and the generalized distance between them computed, giving the calculated difference in "semantic" profiles between them. Alternatively, the same "concept" may be rated by another individual, or the same individual may be retested for the same "concept" at another time, and the distance between profiles may be computed in each case, giving measures of interpersonal agreement on profiles and of change of profiles. No standard list of scales or concepts has been fixed; it is the use of such scales, drawn up for a specific problem to elicit differences between profiles, that gives the procedure the name "semantic differential."[4]

Such tests turn out to be surprisingly reliable. This is the case not only when concepts are rated on "appropriate" scales, such as *feather (light—heavy)*, *lady (smooth—rough)*, etc. (p. 127), but also when seemingly "inappropriate" scales are added: when 112 subjects were retested as to concepts judged on a set of scales of varying "appropriateness," 66% of the answers deviated by less than half a place, and 87% of the answers deviated by less than a full place (p. 138, Table 20, last column; cf. also *Psychological Bulletin* XLIX [1952], p. 229). To be sure, this is a rather gross measure of reliability; only a DETAILED study of the reliability of particular concept ratings on a specific set of scales

[4] By now this term has become familiar in many circles, but it was hardly a happy choice to begin with, as one hardly thinks of a "differential" as an instrument for discovering differences. Could it have been suggested by Korzybski's awkward "structural differential"? Misleading, too, is the free alternation of "incongruous" and "incongruent" as technical terms (e. g., p. 213), and the use of "credulity" for "credibility" with reference to statements.

could be said to measure the degree of interpersonality of the meaning of that concept. Unfortunately, raw reliability data of this latter type are not published; the nearest equivalent are reliability tests of scores on scales already "factor-analyzed" (cf. § 4). These, as a matter of fact, do prove to be very stable, too (pp. 139f.).

3. Representativeness of the Sample Vocabulary. Since the linguist-lexicographer's object of description can be nothing less than the entire vocabulary of a language (or, more precisely, some finite stock of "lexemes" and productive derivation and compounding patterns), very small samples of vocabulary are an undecisive test of a new research approach. More important even than the size of samples is their representativeness. If a doubt arises that the words studied by the new technique may be atypically "investigable" items, it becomes uncertain whether the technique can be extended to the whole vocabulary and, consequently, whether its results are realistic approximations of the lexicographer's goals. Osgood, Suci and Tannenbaum nowhere explicitly propose to begin making dictionaries by their method, but lexicographic relevance is clearly implied in such statements as are quoted in § 1, and it is probably not unfair to examine these implications critically.

In this volume, two fundamental investigations are reported on: one, of 20 concepts rated on 50 scales; another, of 20 concepts on 76 scales. The samples were thus, from a lexicographic point of view, almost laughably small in both series. As to representativeness, however, the two investigations differed markedly:

(a) From Roget's Thesaurus, 289 adjective pairs were selected. Since the electronic brain (ILLIAC) which was to perform the computations has a limited "memory," the list of scales had to be trimmed to 76. This was done by sorting the 289 scales into clusters of similar scales, and selecting one representative scale from each cluster (p. 48). This sampling of the polarities expressible by the stock of English adjectives was at least intended by the authors to be fair and "lexicographically relaistic," and our discussion of their results will therefore be concentrated on this, the Thesaurus study. (Most unfortunately from the present point of view, however, only some phases of the factor analysis [cf. § 4], and not the actual profiles, of the concepts rated in this study are published. The "concepts," judged by 100 subjects, included: *foreigner*, *my mother*, *me*, *Adlai Stevenson*, *knife*, *boulder*, *snow*,

engine, modern art, sin, time, leadership, debate, birth, dawn, symphony, hospital, America, United Nations, family life [p. 49].)

(b) In an association test, subjects were asked to respond to stimuli (common nouns) with descriptive adjectives. These adjectives, apparently supplemented by their antonyms where these were not automatically forthcoming and by the names of three "sensory continua" (such as *pungent—bland*), were arranged as a list of 50 scales (pp. 33f.), which overlaps only slightly with the Thesaurus list. Here the scales were not even meant to be representative of the language as a whole, and there is reason to suspect the representativeness of responses in a test based on something linguistically so primitive as an association test. The results of this, the lexicographically non-realistic study, turned out to yield themselves to factor analysis (see § 4) much better than those of the Thesaurus study. But because this study (which was conducted first) was presumably unrepresentative, the success of ITS factor analysis at best does nothing to recommend the semantic differential to lexicography; at worst—if a causal link exists between its lexicographic unrepresentativeness and its easy factor-analyzability—it serves as a warning that factor analysis is not pertinent to lexicographic research.

Another feature of the samples studied which will make the lexicographer pause is the compound character of many "concepts." We find not only such items as *modern art* and *family life*, but also more complex phrases, e.g. *flexible price supports* or *recognition of Red China*. Clearly the linguist-lexicographer would face an infinite task if the objects of his description had to be phrases rather than simplex words. The inclusion of deictic components (Jespersen's "shifters") in the concepts, e.g. in *my mother* or *my mood today*, also serves to make the task of description coextensive with the infinity of *I*'s and *today*'s. Unless deictic elements can be extracted in a rational way from the "concepts" to be described, and unless the contribution of sub-items to the total meaning of utterances is shown to be finite and computable, there is a serious doubt as to the usefulness of the semantic differential in lexicographic descriptive tasks. (On combinatorial analysis of meanings, see also § 8, toward the end.)

4. FACTOR ANALYSIS. When the rating of concepts on the various scales—76 or 50, as the case may be—was compared, it turned out that some scales showed a fairly high degree of correlation with each other. For example, concepts which are rated

"high" on the *good—bad* scale are often also "high" on the following scales: *beautiful—ugly*, *sweet—sour*, *kind—cruel*, *pleasant—unpleasant*, etc.; concepts which are "high" on the *sharp—dull* scale have a tendency also to be "high" on the *fast—slow* scale, and so on. To analyze the complex correlations of all scales with all others, a statistical procedure called factor analysis is applied.[5] This procedure assumes that there are certain "factors" which, acting as common causes of the variance of concept ratings on the scales, determine to a certain (varying) extent the placement of these concepts on the several scales. For example, in the Thesaurus study the correlations between ratings on the *optimistic—pessimistic* scale and ratings on the *colorful—colorless* scale are such that we may say that some "factor" which determines, with a weight of 37%, the placement of concepts between *optimistic...* and *...pessimistic* also determines, with a weight of 20%, the placement of concepts on the *colorful—colorless* scale; with a weight of 16%, on the *stable—changeable* scale; with a weight of 9%, on the *objective—subjective* scale. This "factor" plays virtually no role, however, in determining the placement of concepts of the *fast—slow* or the *ornate—plain* scales, while it alone completely determines judgments as to *good—bad*, and may for that reason be called the "*good—bad*" factor.

The "factors" extracted by this analysis may or may not be identical with any of the scales themselves, i.e. there may be some factors which do not determine 100% the placement of concepts on any particular scale tested. Thus, in the Thesaurus study, that factor which is 92% determining in judgments on the *sober—drunk* scale is not 100% determining on ANY specific scale that has been tested. The analyst tries, by choosing between "realistic" factors (which correspond to real-language scales) and between "abstract" factors (mere constructs), to account for the variance of judgments in the most efficient way. But no matter how he juggles "realistic" against "constructed" factors, there comes a point, in analyzing non-homogeneous material, where each additional factor extracted accounts for a very small proportion of the variance.

Osgood and his colleagues have applied a factor analysis to both sets of data—those based on Thesaurus scales and those based on association scales. The Thesaurus set being the

[5] Linguists may find it profitable to consult Wells' article cited in footnote 3 for a description of this technique.

lexicographically more interesting one, let us look at its results. A first factor was found which accounts, to a considerable degree, for judgments on the following tested scales: *good—bad*, *kind—cruel*, *grateful—ungrateful*, *harmonious—dissonant*, *beautiful—ugly*, *successful—unsuccessful*, *true—false*, *positive—negative*, *reputable—disreputable*, *wise—foolish*. It is accordingly dubbed the "evaluative factor." A second factor, displaying high "loadings" on such scales as *hard—soft* (.97 loading), *masculine—feminine*, *severe—lenient*, *strong—weak*, *tenacious—yielding*, *heavy—light*, and *mature—youthful*, is called a "potency factor." Factor III is identifiable as an "oriented activity factor," with high loadings on such scales as *active—passive* (.98 loading), *fast—slow*, *excitable—calm*, *rash—cautious*, and *heretical—orthodox*. Since a similar set of three first factors had also been found in judgments on the association-derived scales (which were studied first), these were considered basic. After a slight switch of procedure, the computer extracted five further factors corresponding very nearly to existing adjective-determined scales: IV, a "stability factor," with high loadings on *sober—drunk* (.92), *stable—changeable*, etc.; V, a "tautness factor," with high loadings on *angular—rounded* (.95), *straight—curved*, etc.; VI, a "novelty factor," with high loadings on *new—old* (.97), *usual—unusual*, etc.; VII, a "receptivity factor," and VIII, an "aggressiveness factor."

5. Evaluation of the Factor Analysis. The effectiveness of a factor analysis can be assessed from the proportion of the total variance of the data that is accounted for by the factors extracted. The closer this proportion is to 100%, the more effective the analysis. In the present case, what proportion of the total variance of concept ratings on the numerous scales is accounted for by the hypothesis that the ratings are determined by the eight factors? Surprisingly and most unfortunately, this figure is not given for the Thesaurus study. We are given only the weights of the factors relative to each other (p. 61): we read that Factor I is twice as weighty as Factor II, 3½ times as weighty as III, nearly five times as weighty as IV, and about seven times as weighty as each of Factors V—VIII. But what their combined explanatory power is, we do not know.[6] For details, see also (a) below.

[6] We do learn (p. 62) that Factor VIII, which accounts for as much as .624 of the common variance, accounts for only .005 of the total variance. Proportionately, it would appear that only about .08 of the total variance is accounted for by the factor analysis.

Another way of considering the question of effectiveness is by means of the striking geometric metaphor used by the authors. We might imagine all possible concepts to be located in a space of a certain number of dimensions. A pair of antonymous adjectives may be said to define a dimension in such a space. The location of a particular concept may then be specified by its distance from the origin (center) of the space, i.e. the neutral position, either "upwards" or "downwards" along each dimension. For example, on the basis of semantic-differential data, it may be said that the meaning of *quicksand* is a point in the semantic space 3 units from the origin toward *bad*, 3 units from the origin toward *large*, 2 units toward *stale*, 1 unit toward *soft*, 3 units toward *cold*, 2 units toward *relaxed*, etc. (*Psycholinguistics*, p. 179)—for as many dimensions as the space contains. We do not yet know how many independent adjective-defined dimensions the "conceptual" space of English, or any other language, does in fact contain, or whether such a space is even describable. But if the factor analysis of the scales could successfully attribute the variance of judgments to a small number of factors, we would be approaching the definition of a space of relatively FEW independent dimensions (each dimension corresponding to a factor) and a highly economical placement of concepts as points in that space. The value of the venture thus seems to depend on the success achieved in reducing the hundreds of mutually dependent adjective-defined dimensions to a handful of independent factor-defined dimensions. Moreover, under the added requirements of lexicographic realism, the analysis is successful only insofar as it can be thought of as a sample study of the entire vocabulary.

Taking a linguistically realistic view defined in this way, we again center our attention, for reasons already mentioned, on the "Thesaurus study." We find that the authors' attempt to reduce the semantic space by factor analysis is hardly successful.

(a) *Low Proportion of Overall Variance Explained.* The eight factors extracted in the Thesaurus study account for an unknown, but small, proportion of the actual variance (less than half, and perhaps as little as one-twelfth). The resulting 8-dimensional "map" is thus only a crude projection of a space of a far higher, and unknown, number of dimensions.

This criticism may perhaps be clarified by an analogy. Suppose we were trying to determine the location of various offices on the island of Manhattan. We find that people describe their

locations by phrases such as "nearby," "over there," "near the Battery," "upstairs," "near the George Washington Bridge," "to the west," "three floors down," "used to be right here," "close the waterfront," "six blocks south," etc. We then perform a factor analysis, which discloses the existence of a factor that has high loadings on such descriptions as "near the Battery," "near the George Washington Bridge," and "six blocks to the south," but not on others; another factor accounts fairly well for such distinctions as "near the George Washington Bridge" and "to the west." We then describe Manhattan as a space of an unknown number of dimensions, in which locations may be at least approximatively specified in two "main," independent dimensions: *uptown—downtown* (Factor I, latitude), and *East—West* (Factor II, longitude). But clearly the omission of other "specific" dimensions, such as altitude, which applies to multistoried buildings, or time, which is crucial for buildings no longer or not yet in existence, is a major defect of the description, an inevitable consequence of mapping a multi-(four-) dimensional space in fewer (two) dimensions.

(b) *Low Proportion of Explained Variance, By Scales.* The variance of concept ratings on particular scales is accounted for unevenly, and on the whole quite poorly, by the eight extracted factors, as is evident from the following table (based on Table 5 of the book under review):

TABLE A

Proportion of Variance Extracted of a Scale (h^2)	*Number of Such Scales*
1.00—	8
.61—.99	0
.41—.60	2
.21—.40	24
0—.20	42
Total	76

The eight scales which are fully (1.00) accounted for by the eight factors are those which were arbitrarily selected for just that purpose as "pivots." Of the remaining 68 scales, two-thirds are exhausted by the eight factors to an extent of 20% or less; and almost none are exhausted more fully than 61%. The average is only about 32%.

(c) *Low Proportion of Variance Explained, by Concepts.* Still

more specifically, we may look at the effectiveness of the first FIVE factors alone in accounting for the total variance of 19 concepts in the Thesaurus study, summing over the several scales. (Here, h^2 is computed as the sum of the squares of the "per cent V" scores given by the authors in their Table 30.)

TABLE B

Proportion of a Concept's Variance Accounted For	*Number of Such Concepts*
.16 or less	none
.17—.20	2
.21—.30	11
.31—.34	6
.35 or more	none
Total	19

The average variance extracted from a concept by five factors is here about 27%. The proportion of the total variance accounted for by the first factor on the average concept is only 13% (high, 21.7%—*United Nations;* low, 6.0%—*time, engine*). The corresponding proportion for factors I—V together is, in the average concept, only 26.5% (high, 33.5%—*mother;* low, 17.7%—*time*). All these proportions, it is submitted, are quite low.

(d) *Unknown Reliability of Some Factors.* None but the first three factors have been checked for reliability (p. 74); yet these certainly account for only two-thirds of the common (explained) variance.

(e) *Distortion Due to Sampling.* At this point we must reconsider the manner in which the Thesaurus scales were sampled (cf. § 3a). The 76 scales tested represent, in a way which was meant to be fair, a larger set of 244 scales. For example, *unusual—usual* is treated as representative of a cluster of scales which also includes *eccentric—conventional, impossible—possible, improbable—probable, uncertain—certain, absurd—axiomatic,* and *infrequent—frequent* (p. 59). But 45 Thesaurus scales (i.e., the difference between 289 and 244) have mysteriously disappeared altogether; one searches in vain for "representative samplings" of such scales as *yellow—green, loud—soft, empty—full,* etc. Now, clearly, if eight factors account for only a certain part (say one-half, or one-fourth, or one-twelfth) of the total variance of judgments on the sample scales, then they would account for a much smaller

proportion still on that untrimmed set of scales which the linguist-lexicographer is ultimately interested in. This reduces the success of the analysis even further.

The authors themselves are quite modest in their claims for their instrument; they repeatedly admit that "what we have called the three dominant factors do not exhaust the dimensions along which meaningful judgments are differentiated" (p. 72) and that "a large portion of the total variance remains unaccounted for" (p. 325).[7] What runs afool of linguistic-lexicographic skepticism is their far weaker claim that "three factors appear to be DOMINANT, appearing in most of the analyses made" (p. 72) or that "the same three factors... have reappeared in a wide variety of judgments," and that the "naïve model" for explaining human semantic judgments "in terms of a relatively small number of orthogonal factors" is surprisingly "close to the truth" (p. 328). To be sure, three reliable factors did appear to be "dominant" in the study based on association-derived scales,[8] but this makes the analysis all the more suspect from a global lexicographic standpoint. Responses in association tests are generally simpler than the stimuli (e.g., they are generally higher-frequency words than the stimuli), and we expect them to have little classificatory power when applied to the universe of "concepts." There is no reason *a priori* to expect that factors appearing in such tests should be effective in accounting for the stock of adjective scales of the full language. The book itself seems to show that they are NOT in fact effective in this sense.

Where then does the authors' enthusiasm for their own results

[7] These judicious statements are, however, contradicted by a more extravagant claim (p. 75) that the existence of dimensions beyond the first seven "is not disastrous as far as measurement is concerned ... because these added dimensions account for relatively little [!] of the total variance."

[8] In the study based on association-derived scales, the proportion of variance of each scale extracted by the first four factors is much higher:

TABLE B

h^2 *per Scale*	*Number of Such Scales*
.20 or less	4
.21--,40	14
.41--.60	15
.61--.80	15
.81--1.00	2
Total	50

The average per scale is approximately .55. This is an impressively better showing than in the Thesaurus study.

come from—an enthusiasm that is so uninfectious to a linguist? We may understand it if we consider that the authors were first impressed with the exhaustiveness of a 3-dimensional description of color; that they were then encouraged by the discovery of "factors" in certain studies of synaesthesia, of social stereotypes, and of political opinion. But although the subsequent exhaustion of 50% of the variance in the first factor analysis, based on association-derived scales, was understandably an exciting discovery, there is little justification for considering the far weaker results of the Thesaurus study, distorted as they are by sampling, as "confirmation" of a general theory of semantic space. On the contrary, it would seem proper to conclude that in the vocabulary of a language as a whole, the "first three factors" account for a not-too-significant part of semantic judgment. And since further analysis, by the authors' own admission, yields no good general factors (pp. 36, 62), it seems fair to conclude that the method of factor analysis does NOT represent a major breakthrough in descriptive semantics. (But see § 9, (a) and (b).)

6. A QUESTION OF VALIDITY. The authors display a great deal of concern with the "validity" of their instrument, i.e. with the ability of the semantic differential to yield measurements that correlate well with scores obtained in some other, independent way. A study is cited in which ten "concepts" *(white rose buds, gentleness, sleep; hero, virility, success; quicksand, fate, death; methodology)* were presented to 160 subjects for rating on a 20-scale version of the semantic differential. When the results were actually mapped in a 3-dimensional model, they turned out to arrange themselves in three clusters of three concepts each, and one cluster of a single concept, as separated by the semicolons in the above listing. This arrangement, the authors argue, has a high "face validity," since "most people[9] would have clustered these concepts in much the same way without using the differential" (p. 141). But this argument is subject to several important limitations.

(a) The ten concepts can hardly be called a representative sample of our noun vocabulary; they seem definitely "pre-clustered." A more diversified group, such as *knife, gentleness, pepper, methodology, bookbinding*, would hardly lend itself to easy

[9] That is, most Americans. As Peter R. Hofstaetter has suggested ("Ueber Aehnlichkeit," *Psyche* I [1955], 54-80), Europeans would probably put *hero* nearer to *tragedy* than to *success*.

clustering. How would it come out on the differential? Also, the sample is quite small. It is easy to represent four points (corresponding to the four clusters) as equidistant from each other in a 3-dimensional space, but more than four mutually equidistant points could not be accommodated in three dimensions. How would THEY be distributed by the differential?

(b) It is difficult to judge the validity of the representation in detail. The space model makes sense only if the angle formed by the short line within the cluster, e.g. from *death* to *quicksand*, and the long line from that cluster to the *gentleness* cluster, is determinate. From the model (p. 95), they appear to be parallel, yet it is virtually impossible to judge intuitively whether the semantic differences form a proportion, i.e. whether *death* is really to *quicksand* as their common semantic denominator is to *gentleness*.

(c) The semantic differential underlying the model is based on nine scales, selected from the association-derived set because they display atypically high loadings of the first three factors. This is a modification of the instrument in a direction opposite to lexicographic realism.

7. BUT IS IT MEANING? At this point we are ready to consider the question of what the semantic differential really measures. The authors consider it "a psycholinguistic tool, designed... to measure the meanings ... of signs" (p. 275). It measures "meaning" in a particular psychological sense, which they take pains to define.[10] But they are unfortunately rather helpless in placing their position in a historical framework of a general theory of signification. As futile examples of linguists' views on meaning, they cite only a few Americans (Bloomfield, Harris, Joos)—men who are well known to have dogmatically rejected an investigation of semantic problems as such. A reading even of an elementary and relatively conservative presentation, such as Ullmann's *Principles of Semantics*, or of Morris' *Foundations of a*

[10] "That process or state in the behavior of a sign-using organism which is assumed to be a necessary consequence of the reception of sign-stimuli and a necessary antecedent for the production of sign-responses" (p. 9). This process (a) is "some fractional part of the total behavior elicited by the significate," and (b) it produces "responses which would not occur without the previous contiguity of non-significate and significate patterns of stimulation" (p. 7). However, "the vast majority of signs used in ordinary communication are what we may term *assigns*—their meanings are literally 'assigned' to them via association with other signs" (p. 8).

Theory of Signs, would probably have made the authors more fully aware of the conventional forms of the problem of meaning and would have provided them with a more adequate terminology for the discussion of the theoretical questions. They would in any case have found reasons to avoid so crude a dichotomy as "what has variously been called denotative, designative or referential 'meaning' [on the one hand] and ... connotative, emotive, or metaphorical 'meaning'" (p. 321) on the other, in which terms culled from various writers and from mutually incompatible frames of reference are lumped together.

Osgood, Suci and Tannenbaum claim that "the semantic differential taps the connotative aspects of meaning more immediately than the highly diversified denotative aspects" (p. 290). The authors cannot be using "connotation" in its obsolescent technical sense as developed by J. S. Mill;[11] for on that interpretation, they are investigating NEITHER denotation (i.e. reference, extension, relations between signs and things) NOR connotation (i.e. signification, intension, conditions which must be satisfied if a sign is to denote). What the semantic differential IS equipped to measure seems to be some aspect of the affect of words, their so-called "emotive influence,"[12] their power to produce extra-linguistic emotional reactions. These are perhaps "connotations" in the loose, non-technical sense of the word (of which, incidentally, Bloomfield was also guilty). But by the authors' statement, these "connotations" have (literally!) nothing to do with the referential capabilities or functions of signs.[13] In fact, affective powers can be found, and can be measured by this instrument, in nonsensical phoneme sequences as well as in meaningful ones (p. 287), in pictures as well as in words (p. 68), in non-signs—noises (p. 66) and kitchen

[11] "Connotation: the meaning of a term defined by abstract qualities common to a class of objects or instances designated by that term.... The connotation [of "U. S. citizen"] comprises the characteristics of the American citizen and the rights, privileges, and duties conferred by citizenship." H. B. and A. C. English, *A Comprehensive Dictionary of Psychological and Psychoanalytic Terms*, New York, 1958, pp. 111, 144.

[12] Cf. Max Black, *Language and Philosophy*, Ithaca, 1949, p. 219, where the internal contradictions of the phrase "emotive meaning" are discussed.

[13] "Agreement on the referents of signs," the authors write, "implies nothing whatsoever [!] about similarity of the representation states [i. e. what the semantic differential measures] associated with these signs" (p. 323). "We must admit that distances in our semantic space as between individuals judging the same concepts are *not* indicative of degrees of *referential* agreement" (p. 322). It follows that distances between "concepts" as judged by an individual do not correspond to referential differences, either.

smells and decorators' color schemes—as well as in signs. The "psychological states" whose projections the semantic differential is out to capture occur, no doubt, in speech behavior too, but such emotive influence is an aspect of all experience and all behavior, and is not restricted to language or even to communication in general, in which meaning (signification and reference) are distinctive components.[14]

The intimate relations between meaning and affect deserve systematic investigation, but the first step should be a theoretical distinction between them. Had the authors taken such a step, they would not in their tests have tenaciously retained scales for which factor analysis is manifestly ineffective (e.g., in the study of association-derived scales, *wet—dry*, with an h^2 of .03; *yellow—blue*, with an h^2 of .17; and so on). They would have capitalized on the excellent reliability of the evaluative factor (as a measure of affect or attitude[15]) in contrast with the relatively poor performance of all other factors, which seem to have progressively less and less to do with affect, i.e. with what the semantic differential measures. They would have refrained from pressing subjects for judgments of concepts on demonstrably inappropriate scales.[16] In sum, the reluctance of Osgood and his associates to distinguish meaning from affect makes it difficult, from a linguistic-lexicographic point of view, to sort out their possible successes with respect to affect from their evident or suspected failures with respect to meaning.

8. The Algebra of Affect. It is also useful to disentagle meaning proper from affect in discussing the authors' attempt at a combinatorial algebra of "meaning." In one section of the book they concern themselves with "the manner in which meanings

[14] For an excellent discussion of the irrelevance of (lay or Bloomfieldian) "connotation" to meaning, see now Holger Steen Sørensen, *Word-Classes in Modern English*, Copenhagen, 1958, § 17.

[15] On p. 189, the authors appear explicitly to identify "evaluation" with "attitude" by stating that "it has been feasible to identify 'attitude' as one of the major dimensions of meaning-in-general."

[16] If *warm* is put at the midpoint of the *hot—cold* scale, this is probably a valid SEMANTIC judgment on an appropriate scale. Similarly, foreigner may occupy an AFFECTIVE midpoint on a *good—bad* scale. But the midpoint location of, say, *boulder* on the *sweet—sour* scale is not due to either semantic or affective judgments, but the inappropriateness of the scale to the concept. An investigation to be productive should include these considerations as axioms; subjects should be free to select and reject scales.

interact and are thereby changed" (p. 200). "What happens," they ask, "when two (or more) signs are presented simultaneously, e.g. when the subject sees the phrase *lazy athlete*? Common sense tells us that some interaction takes place—certainly a *lazy* athlete is much less active, perhaps less potent, and probably less valuable than he would be otherwise." Languages, they argue, have devices for bringing signs into a "peculiar evaluative relation to one another that we shall call an assertion" (p. 201). While they have no "precise definition" of this notion and "have not been able to make explicit the criteria on [*sic*] which [they] operate' (p. 202), they do list, among the forms of assertion, "simple linguistic qualification" of nouns by adjectives and verbs by adverbs, "statements of classification" (e.g. *Tom is an ex-con* or *Cigarettes contain nicotine*) and those "source-object assertions" which are often studied in opinion and attitude research (e.g. *Communists dislike strong labor unions*). Assertions are said to be "either *associative* or *dissociative*, which corresponds to the basic distinction in all languages between affirmation and negation" (p. 201). A "principle of congruity" is developed which states that when two signs are related in an assertion, the "mediating reaction" of each is adjusted to the other, i.e. if *Eisenhower* is favorably viewed, and one is exposed to the assertion *Eisenhower praises new educational policy*, one's evaluation of *new educational policy* shifts in a favorable direction related to that of *Eisenhower.* This is shown to operate in attitude measurement, but no evidence is given that anything except positive-negative evaluation (equivalent to Factor I) is subject to the "principle of congruity." These investigations do not therefore in any proven or promising sense contribute answers to the original question about "the manner in which MEANINGS interact."

A more direct attack on combinations in a linguistically interesting sense may be found in the investigation of Osgood and D. C. Ferguson of "semantic effects of word combination" (pp. 275ff.). Eight adjectives were combined with eight nouns into all possible 64 phrases (rather quaintly called "word mixtures"), e.g. *listless nurse*, *shy nurse*, *listless comedian*, etc. Ratings of constituents as well as phrases on a nine-scale differential were obtained, but only the first three factors were considered. The linguistic significance of the study is hard to assess, since the actual data for only one phrase *(shy secretary)* are published. (Yet almost every combination raises interesting separate problems. For example, whatever the semantic profile

of *average*, one would expect that this, of all adjectives, is the modifier which would have a null effect on the profile of the noun it modifies. Such information is lost in the summary tabulations.) Still the experiment yielded some interesting and unexpected results, for example: that the adjective exerts a greater affective pull on the phrase than the noun; that derogatory components exert a disproportionately strong pull; that, in general, the scores for the phrases were predicted with remarkable success from the scores of the constituents, the predictions being even better for Factors II and III than for I; and that the less "comparable" two constituents are, i.e. the more disparate their profiles, the more poorly is the profile of the phrase predictable. It would appear, then, that a rough combinatorial formula for calculating the combined AFFECT of some adjective-noun phrases which are affectively harmonious to begin with could be devised; but that for the combinatorial analysis of MEANING proper, the semantic differential is a poor instrument.

Another experiment dealt with the combinatorial properties of "meaning" in the formation of "assigns" (see footnote 10). The study is too crude, from a linguistic point of view, to be considered an approximation of the way in which we understand definitions, for it presupposes only that assigns are "associated" with primary signs (i.e., elements of the definiens), without stating anything about the structure of this association (p. 287), and depending only on the primitive notion of "assertion." That this is an inadequate theory can be seen by examining such assigns as *brother-in-law* and *sister-in-law:* we find them BOTH "associated" with the primary signs *brother*, *wife*, and "possession" (i.e., =*wife's brother* and =*brother's wife*), yet their meanings are distinct and would probably yield different profiles even on the semantic differential. But if we interpret this experiment more modestly, as showing that some elements of affect are conveyed to a newly learned word by the context in which it was learned, the results are interesting and more specific as to the contributing role of the frequencies of repetition of defining terms, the semantic "polarization" (extremeness) of the defining terms, etc., than what the armchair linguist might expect merely by intuition.

It would be fair also to require this algebra of affect to have been applied in reverse as a demonstration that the profiles of non-simple "concepts" such as *white rose buds* or *my mother* are predictable from those of their constituents. This would have been a test of whether the coverage of an entire language, with its

infinity of possible phrases and sentences, by the semantic differential can be reduced to a finite task. This, as was mentioned in § 3 (end), would be an important trial of the relevance of the instrument to linguistic lexicography.

Finally, it may be noted that a clear distinction between signification and affect would have saved the authors from error in their speculations about contextual problems. They ask (p. 323): "How can there be discriminative encoding despite connotative indiscriminability?" That is to say, since concepts like *nurse* and *sincere* "may occupy essentially the same region of our semantic space," why aren't the words in free variation? The answer, according to our authors, is complementary distribution: presumably, we have "habits of usage and association" which provide that when we are in a certain "representational state" x, we use *sincere* in the context *she is*— and *nurse* in the context *she is a*—. This argument is demonstrably absurd, since it will be agreed that it is not self-contradictory to say, *She is sincere but not a nurse*, and that *The nurse is a nurse* is tautologous, but *The nurse is sincere* is not. The absurdity stems from the falsity of the premiss. *Nurse* and *sincere* occupy "essentially the same region" NOT in our semantic space, but in a 3-dimensional map of a space of very many dimensions, a map whose order of crudeness is not even known. If this qualification had been constantly in the author's minds, they would not have ventured the rather preposterous suggestion that affect and linguistic context alone, without signification, are sufficient determinants of the selection and interpretation of signs.

9. VALUE OF NEW TECHNIQUES. What, then, are the contributions of *The Measurement of Meaning* to linguistic lexicography?

(a) The lexicographer must be grateful for the semantic differential, as he must appreciate studies of word association, because of the possibilities it has opened for the systematic description of the "affective" capabilities of words. The day may come when the location of a word in the "affective space" of a language will be included in descriptive dictionaries. This is a promising outlook, because pilot studies already seem to have shown interesting differences between languages. For example, the equivalents of the *rich—poor* and *hot—cold* scales in Korean are apparently more clearly evaluative than in (American) English or in Japanese; the *rugged—delicate* scale has a stronger loading on the "dynamism factor" in (American) English than its equivalents in Japanese and Korean (p. 175); and so on.

(b) The insight that the affective powers of words may have a dimensional structure of a certain kind is in itself interesting and valuable. The conclusion of Osgood and his associates are not as clearcut as they might have been with better theoretical underpinnings, and much ground may have to be gone over again with better preparation. But the findings are already challenging.

(c) The traditional lexicographer's way of arriving at a semantic description of a word, *A*, is to ask himself, "What does *A* mean?" Techniques such as those utilized by Osgood and his collaborators make greater objectivity possible in that the describer, instead of asking himself, asks a representative sample of the speech community, and treats the degree of agreement between answers as a significant and measurable variable. In order to keep answers from varying too wildly and to make them suitable for quantitative analysis, the technique prescribes that the subjects make a multiple choice from among a preselected set of possible answers. The resulting quasi-semantic description is then condensed further, by means of statistical manipulation. All these are features which an experimental lexicography may in future want to adopt. The glaring disadvantages of the semantic differential are the lexicological and psychological inappropriateness of some of the preselected answers from which the subject is forced to choose, and the relative ineffectiveness of the condensation achieved by the particular analysis in terms of "factors."

Would it be possible for lexicography to exploit the advantages of objectivity, quantifiability, and condensation while eliminating the disadvantages of inappropriateness and crudeness of mapping? Perhaps it would. One thinks in this connection of the game "Twenty Questions," to which the authors refer (p. 19) as an implicit analog of the semantic differential. Very little seems to be known about the strategy involved in the game and about the structure of our languages and our conceptual universe as revealed by the game.[17] Yet the game can be played and won, and it

[11] In this game, familiar to American radio and television audiences, Opponent I (the quizmaster) thinks of a "concept" and tells Opponent II (the panel) whether it is animal, vegetable, or mineral, or any combination of these. The panel is entitled to ask twenty questions (e. g., "Is it living?", "Is it human?", "Is it manufactured?", etc.) which are answered by Opponent I by "Yes," "No," or "Sometimes (= Partly)." In some but not in all cases, Opponent II is able to guess the correct concept. —The few available studies, such as D. W. Taylor and W. L. Faust, "Twenty Questions: Efficiency in Problem Solving as a Function of Size a Group," *Journal of Experimental*

yields recognizable semantic descriptions compared to which the specifications of concepts obtained by the semantic differential seem hopelessly obscure.[18] The game has its limitations as a tool for semantic description: as ordinarily played, its "targets" are restricted to "concrete" noun. But it is worth considering whether a formalized investigation replicating the game would not produce a valid and economical description of a vocabulary from the point of view of signification.

The universe of concepts revealed by "Twenty Questions" seems offhand to be describable by a branching liagram, or tree, corresponding to a sequence of ever finer binary choices. On inspection it turns out that some questions may be asked in any order, whereas the order of other questions is predetermined. Only to the extent that the order of questions is FREE can the tree model economically be converted into an "*n*-dimensional space" model of the type used by Osgood, Suci, and Tannenbaum. The actual breakdown of their space model soon after one passes from affect to signification proper[19] is to be expected from the fact that the order of questions in a lexicologically realistic inquiry like "Twenty Questions" is very largely fixed. On the other hand, it is conceivable that the binary choices of "Twenty Questions" could, in the interests of economy, in certain cases be replaced by multiple choices, some of which may entail scale ratings.[20] Should

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Psychology XLIV (1952), 360-367, or A. W. Bendig, "Twenty Questions: an Information Analysis," *ibid.* XLVI (1953), 345-348, display no interest in the lexicological structure of English as such.

[18] That is to say, when we specify the meaning of a concept as "animal... human... living... male... American... political figure... federal office holder... elective..." etc., we are zeroing in on something (let us say, *Eisenhower*); whereas if we specify a concept (in terms of Osgood-type factors) as "somewhat bad, indifferently potent, and quite passive" (p. 88) or even, in terms of actual adjective-determined scales, as "indifferently angular, quite strong, very smooth, rather active, indifferently small, dry, and cold, very good, quite relaxed, and rather fresh (*Psychologial Bulletin* XLIX [1952], p. 229), we are hardly zeroing in on anything. (The answers are *fate* and *polite*, respectively.)

[19] The quick exhaustion of " general factors" and the need to search for factors specific to particular classes of concepts is the chief manifestation of the breakdown. Note also the splitting up of the eveluative factor into "subfactors" and the mixture of metaphors in describing Factor I as "a sheath with leaves unfolding toward various directions of the total space" (p. 70). The discussion of the possibility of non-Euclidean properties of the "semantic space" (p. 144) seems premature in view of the strictures against the space that is yielded by the factor analysis.

[20] The choices in "Twenty Questions" may not seem truly binary, but the third and fourth possible answers—"Partly (= Sometimes)" and "Not in the usual sense of the word"—are in reality "meta-answers" which do not affect the basic binarity.

that turn out to be the case, the scaling procedures developed in connection with the semantic differential may turn out to be useful supplements to the "grading" experience of linguists like Sapir.[21] Also, investigations need not be restricted to noun-by-adjective matching; noun-noun matchings would surely also the productive.

As a result of Columbus' voyage across the Atlantic, the inadequate concept of "India" had to be quite drastically differentiated into West Indies and East Indies; yet certain positive discoveries of lasting value were achieved. The travels through semantic space by Osgood and his crew of explorers may necessitate an equally radical differentiation of their "meaning" into signification and affect. But even if they have not found a new passage to India, their navigational experience may yet be useful in unforeseen ways.

Columbia University.

[21] Edward Sapir, "Grading: a Study in Semantics," in *Selected Writings of Edward Sapir...*, ed. D. G. Mandelbaum, Berkeley and Los Angeles, 1949, pp. 122-149.

CREOLIZED LANGUAGES AND "GENETIC RELATIONSHIPS"

ROBERT A. HALL JR.

In his article "Language Contacts in the British West Indies" (*Word* 12.399-414 [1956]), and especially in Part II, "On the Classification of Creolized Languages" (407-414), Douglas Taylor discusses certain points that I tried to make in my article "The Genetic Relationships of Haitian Creole" (*Ricerche linguistiche*, 1.194-203 [1950]) and in my monograph *Haitian Creole: Grammar, Texts, Vocabulary* (American Anthropological Association, 1953). There are two aspects of Taylor's discussion that merit further clarification: the nature of the relationship between Haitian and other Creoles and French, and the criteria to be used in determining such relationship, especially with regard to "structure."

In my article and monograph, I had used the term "genetic relationships," suggesting that Haitian and other Creoles were to be considered as "genetically" derived from French, not from African languages, though in no wise denying strong structural influence from these latter. It did not give a detailed definition of the term "genetic relationship," particularly with regard to the length of time or the continuity of transmission presumed to be involved, but used this term in the general acceptance it has among American linguistic analysts. On p. 407 of his article, Taylor quotes a passage from Meillet's *Introduction* in which Meillet insists on continuity of transmission from one generation to another within the same speech-community, and on gradualness of change, as criteria for defining linguistic evolution and hence "genetic" relationship. Taylor then says: "Now if this and nothing else is to be called genetic relatedness in the linguistic sense—and different things should not go under the same name—then the criteria by which it is to be recognized must be such as will exclude any pidgin, lingua franca or creolized language from

that relationship with languages other than those to which, by splitting into different languages, it may itself give rise." But who decided that "this and nothing else is to be called genetic relatedness?" Certainly Meillet's insistence on gradual, generation-to-generation evolution is not of the essence of genetic relatedness, and is not shared by all linguistic historians. One of the main aims of my discussion in *Ricerche linguistiche* and in my article "Sostrato e Lingue Créole" in the *Archivio glottologico italiano* 40.1-9 (1955) was to suggest that for genetic relationship as we customarily use the term, it is irrelevant whether we consider the linguistic changes involved to have taken place gradually or in a saltatory manner, or to have been transmitted from generation to generation or not. Meillet's definition of genetic relationships is narrow, incomplete and vague: does he, in speaking of "l'effort fait par l'enfant pour reproduire la langue des adultes," take into account the existence of baby-talk used by adults to children, which differs markedly from the normal adult speech which Meillet seems to think the only goal of the child's imitation? Certainly it does not seem reasonable to take one particular definition of a term as one's standard, and to reject any other use of the term simply because it does not conform to the one chosen arbitrarily.

As for the actual type of relationship involved, that which I was referring to by the term "genetic" (and which I believe most present-day historians of language would understand by this term) was the type referred to by Kent *(Sounds of Latin)* and Bloomfield *(Language)*: when we find systematic correspondences in all aspects of language structure (phonology, morphology, syntax, vocabulary) between two languages attested contemporaneously or at different stages of time, we conclude that the one must be a later stage of the other, or else that they must have come from a common source. Note that, on the one hand, this says nothing concerning the means of transmission from one generation to another or from one speech-community to another (as when, for instance, speakers of Etruscan or Gaulish or Iberian learned Latin). On the other hand, it does emphasize that there must be correspondences between all levels of linguistic structure, not merely in phonology and vocabulary (which latter is one of the least reliable indications of linguistic relationship), and that these correspondences must be systematic (rather than merely numerous).

From the languages on which they have traditionally worked, and for which they have reconstructed "proto-languages" (e.g.

Indo-European, Malayo-Polynesian, Finno-Ugric), comparativists have customarily made certain further, corollary assumptions, which are the immediate sources of disagreement: (a) that, among languages related through having come from a common source, the process of differentiation has always been gradual; and (b) that, among such languages, the relationship has always been "pure," i.e. with little or no introduction of structural patterns (phonemic, morphological, syntactical) from any source outside the language family concerned. This latter corollary is essentially a survival from Renaissance and neo-classical purism; the extreme case of "historical purism," as we might call it, was the remark made by some scholar that a certain Old Persian inscription containing *būmā* 'earth' instead of the "correct" *būmī* must necessarily have been a later forgery, since no pure-blooded Persian would have been so false to his ancestral language!

Even before the current intensification of work on pidgins and creoles, various scholars had expressed doubts as to the validity of these corollary assumptions, and from the evidence of pidgins and creoles it is quite clear that these doubts are justified. The re-structuring of English into South Sea Pidgin (from which Neo-Melanesian, Neo-Solomonic and the other pidgins of Oceania developed) took place in not over half a century, from the 1820's (when speakers of English first came in effective numbers into the South Sea Islands) to 1860 or 1870; likewise, that of English into "Chinese Pidgin" English took place in a short time, from the 1660's (when the first "factory" was opened at Canton) until the first part of the eighteenth century, when our first attestations of CPE occur; and the Pidgin French out of which the various Creoles grew must have arisen in a similarly short period. Half a century is, if anything, a long time, in the time-perspective of pidgin languages: a pidgin can grow up in a few days of trading, or in a few hours of contact between an English-speaking tourist and an Italian cicerone. The re-structurings which take place in these instances are very brusque and violent, and take place very fast, as compared with what we customarily consider "gradual" change over the centuries. Furthermore, no linguistic relationship is "pure," whether the change be gradual or brusque; just to remain on familiar ground, we have plenty of instances of Romance languages taking over structural features from territorially neighboring languages (e.g. Rumanian getting /i/ from Slavic) and vice versa (e.g. Alsatian German getting nasalized vowels from French).

Yet in these latter instances, no one considers that the basic relationship of the language is changed by the presence of structural borrowings; we do not classify Alsatian German with the Romance languages because it has nasalized vowels. The question now arises: how far can structural borrowings go before they affect our classification of a language? With the traditional type of comparativism, the question did not arise, because it was assumed that genetic relationships were "pure," and that hence a given language must be related genetically to one and only one language family. In order to combat the comparative method, Schuchardt and his followers opposed the concept of "mixed" language to that of "pure" language, and Schuchardt's studies on pidgins and creoles were undertaken primarily for the purpose of demonstrating that these, as "mixed" languages, were related to more than one family, and that therefore the entire concept of necessary genetic relationship, which they thought to be based on linguistic "purity," must perforce be untenable. Since, however, we have by now abandoned as unfruitful the opposition between "pure" and "mixed" languages (we now recognize that all languages are "impure"), the immediate aim of Schuchardt's approach to pidgins and creoles becomes invalid.

But even though all languages are "mixed," some are, so to speak, more "mixed" than others. We are left with the question of whether, in fact, the more mixed languages are so mixed as to invalidate the assumption of genetic relationship, particularly as applied to languages of whose detailed history we have no knowledge. In theory, a language might conceivably combine elements from two or more sources so that they were perfectly evenly balanced, and so that they would be, therefore, unclassifiable according to our customary assumption. Yet my contention is that, in practice, such a condition of perfect balance is never found, not even in any of the pidgins and creoles that have been investigated in more detail than, say, Schuchardt was able to do, and not even with their admittedly extensive carry-overs in structure as well as in vocabulary from Chinese, Melanesian, African, and other substrata. My contention is furthermore that the proportion of French structure in Creole is both greater and more fundamental than that of African structure, and similarly for Chinese Pidgin, Neo-Melanesian, Sranan-Tongo, etc., in relation to English. Even Esperanto is basically an artificial Pidgin Romance, with some lexical and structural borrowings from Germanic and other sources.

At this point, the term "structure" needs further clarification.

Taylor also speaks of "structure" as a criterion for determining linguistic relationships, but in such a way as to imply that "structure" is equivalent to morphology and syntax, not to phonology, as where he says (p. 408) that Voorhoeve's and Sylvain's classifications "admittedly were based, in the main, on structural similarities; while those of Hall seem principally to rely on vocabulary and what appear to be regular phonetic correspondences." This does not seem to me to represent my position accurately: I certainly do not rely on mere vocabulary (a large part of which, particularly in the modern Haitian Creole texts that were available to me, is obviously recently borrowed from standard French), nor yet only on regular phonetic correspondences. It seems to me that the systematic correspondences between Creole and French, or between Sranan-Tongo or Neo-Melanesian and English, etc., are all-pervasive, i.e. found in all branches of linguistic structure, whereas the correspondences between Creole and Sranan-Tongo on the one hand, and African languages on the other hand (or between Neo-Melanesian and Melanesian languages, between "Chinese Pidgin" English and Chinese, etc.) are indeed extensive, but scattered and less systematic. (At this point, this or any similar statement has to be impressionistic; we shall have to wait for a full historical grammar of one of the Creoles in order to discuss it in even approximately quantitative terms.)

Furthermore, it seems to me that, in his discussion of morphological and syntactical parallels between Creole and Sranan-Tongo on the one hand, and African languages on the other, Taylor does not distinguish sufficiently between different levels of strcture in morphology and syntax. Of course quite a number of Creole inflectional categories and grammatical processes correspond to African rather than to Romance patterns; to deny this, it would take an almost Fainesque dilettantism. Yet there are different levels of structure involved in, say, specific inflectional categories and processes, as opposed to the fundamental form-classes of the language as a whole, and the latter are surely more basic than the former. Similarly, derivational patterns are more basic, closer to the core of linguistic structure, than are inflectional patterns. Thus, for example, if the facts of the language force us to recognize Romance-type conjugational differences in verbs on the derivational rather than the inflectional level, this leads us to the conclusion that the Romance origin of the verb system is still evident, but has been pushed down to a more fundamental but less obvious level of linguistic structure by

later accretions from other (African) sources. In syntax, likewise, Taylor finds "slight and unconvincing" the replaceability of word by word-group and the fixed order subject+predicate; to me these seem weighty and convincing because they are on a more basic level, they are more part of underlying pattern, than are single combination-types such as we get in one kind of phrase or another—even though these latter may be more numerous in a list-count. (Here, too, systematicity and pattern-congruity are more important than mere number.)

As for the term "genetic relationship" itself, I hold no special brief for it. If we dislike it (as, say, Hockett also does, in his recent article "The Terminology of Historical Linguistics", *Studies in Linguistics* 12.57-73 [1957]) and want to substitute for it another term, well and good. If we want to use the terms "basic" and "lexical," all right; in those terms, however, I should be inclined to say that the "basic" relationship of Creole is with seventeenth-century French, with heavy carry-overs or survivals of African linguistic structure (on a more superficial structural level) from the previous language(s) of the earliest speakers of Negro Pidgin French; and that its "lexical" relationship is with nineteenth- and twentieth-century French. I should further deny a "basic" relationship, in Taylor's sense, between Creole and Sranan-Tongo, Gullah, Papiamentu, etc., in that the undoubted similarities between them are to be ascribed to carry-overs from a common African substratum, not to a common source of fundamental and all-pervasive linguistic structure. Similarly, we do not consider Albanian a Romance language, nor Chamorro a dialect of Spanish. We do not consider English, French, and North Italian to be all "basically" derived from Celtic because we may deem them to have more or less extensive structural survivals or carry-overs from a common Celtic substratum; and it would be unfruitful to call, say, French "a Celtic language spoken with Latin words," in that such a formulation obscures the more fundamental structural relationship of French to Latin and Romance. Nor does it matter that the continuity of the speech-community was interrupted; of course a brand-new speech-community was formed every time a new pidgin was developed, but the speech-community is one thing (a grouping of people) and the linguistic structure is something totally different (a patterning of linguistic habits).

It all boils down, so far as the linguistic relationships are concerned, to the relative weight we wish to put on different struc-

tural features. I seem to be in the middle between such scholars as Voorhoeve, Sylvain (at least in her 1936 monograph) and Taylor, whose descriptions of Creole are scientifically based, but who over-emphasize the rôle of the substratum in the same way that Schuchardt did; and on the other hand such persons as J. Faine and most Romance scholars, with whom one has to strive to convince them of the existence of any African substratum at all. For the general theory of linguistic relationships, the case of creolized languages seems to me to confirm, not to deny, the usual applicability of what we have been accustomed to call the hypothesis of necessary "genetic relationship" among languages.

In any case, both Taylor (to whom I am grateful for extensive discussion of this article in earlier drafts) and I agree in hoping that this friendly divergence of viewpoints will stimulate further discussion, for the subject is of too great an interest to be confined to an argument between two persons.

Cornell University.

ON THE COMPATIBILITY OF GENETIC RELATIONSHIP AND CONVERGENT DEVELOPMENT

URIEL WEINREICH

In the discussion between Messrs. Taylor and Hall, which is a pleasant echo of an older debate between Schuchardt and Meillet,[1] several issues may, I feel, be resolved by a more rigorous formulation of the problem and a more faithful recourse to authorities.

1. MEILLET'S POSITION. I do not agree with Mr. Hall that Meillet's position is "narrow, incomplete, and vague." Meillet held that:

a. A language may be influenced in its phonological and grammatical structure by another language. This influence, he felt, seldom reaches very far; but even when it is precipitous, as in the case of the Creoles, it does not remotely approach a mixture of two systems on an equal basis (*LHLG* I, 83, 85).

b. A population either continues to speak a language or shifts to another: no third possibility exists. Even the radical changes which French underwent in the Caribbean under "African" influence could, according to Meillet, have been experienced by the speakers as constituting a continued identity of the language (*LHLG* I, 85; *Intro.*, 23f.).

[1] See, for example, A. Meillet, "Le Problème de la parenté des langues," *Scientia (Rivista di scienza)*, XV (1914), 403-425, reprinted in his *Linguistique historique et linguistique générale* (hereinafter *LHLG*), Paris, I, 1921, 76-101; Hugo Schuchardt, "Zur methodischen Erforschung der Sprachwissenschaft, II," *Nordisk Tidsskrift for Filologi*, ser. 4, VI (1914), 145-151, and "Sprachverwandtschaft," *Sitzungsberichte der Berliner Akademie der Wissenschaften*, XXXVII (1917), 518-529, reprinted in *Hugo Schuchardt-Brevier*, Halle, 2nd ed., 1928, 189-204; Meillet, "Les Parentés de langues," *B. S. L. P.*, XXI (1921), 9-15, reprinted in his *LHLG*, 102-109. Meillet's *Introduction à l'étude comparative des langues indo-européennes* (Paris, 1949 printing) is referred to in this note as *Intro.*

c. On the linguistic evidence, the French-based Creoles, then, are without a doubt branches of French (*LHLG* I, 85). Correspondingly, we may infer, Meillet would have viewed Sranan-Tongo as a branch of Dutch, etc.

d. The influence of other structures on the Creoles is nowhere near so strong as to cast doubt on their classification as modern Indo-European. Meillet does admit that "mixtures" such as Slavo-Italian and Slavo-German, described by Schuchardt, fit the theory of unambiguous genetic classification less well; but, opposing Schuchardt, he argues that those "mixtures" are (1) ephemeral, and (2) too poorly described to serve as a basis of new theories (*LHLG* I, 109): "Avant de conclure que la théorie classique de familles de langues ne s'applique pas aux cas obscurs, il faudra établir des grammaires comparées qui ne sont pas faites jusqu'ici."

2. TAYLOR'S POSITION. Mr. Taylor (*Word* 12.407) cites a passage by Meillet (*Intro.*, 20) on the continuity of transmission which characterizes the languages within a genetic family, and then argues that such continuity could not have existed in the transition from French to Creole in the Caribbean. However, (a) Meillet considered such continuity not a *criterion*, but merely a common characteristic of genetic relationship. The cited passage is from an Indo-European comparative grammar, and the formulation is explicitly tailored to fit the well known cases; Meillet himself cautions that "ces principes seront posés d'une manière générale. Toutefois, ils se rapportent avant tout aux langues indo-européennes [anciennes]" (*Intro.*, 14). (b) Nevertheless he shows (*Intro.*, 23f., and especially in *LHLG* I, 85—a paper of far more general scope than *Intro.*) precisely how the changes in Caribbean French can be viewed as fitting into a continuous history of the language. It would seem, therefore, that Mr. Taylor does Meillet an injustice by basing a new theory on a fragment of Meillet's work, taken out of context.

To use relative continuity or discontinuity of transmission as a *criterion*, as Mr. Taylor urges, would probably be unwise, since past circumstances of transmission cannot easily be reconstructed. The structures of the surviving languages are the only solid evidence available, and they are the only safe basis for assignment to a language family. The classification of languages by their functions (e.g. Reinecke's "plantation languages" and "trade

languages,"[2] nursery language, etc.) should be a separate and self-contained procedure.

Mr. Taylor's use (pp. 408f.) of the system proposed by D. H. Hymes (*Word* 11.10-23 [1955]) to show a quasi-genetic relationship of the Creoles to each other is not convincing. Mr. Hymes applied his method to the reconstruction of the pattern of the verb in proto-Athapaskan, but he did not, and could not, use it to prove the existence of the Athapaskan language family. This he had to consider as pre-determined by conventional methods of comparative linguistics.

3. HALL'S POSITION. On the basic issue Mr. Hall is, in my opinion, more nearly right than Mr. Taylor. But it is only Mr. Taylor's out-of-context quotation which lays open Meillet's definition of genetic kinship to the charge (*Word* 14.368) of being vague and unworkable. If Mr. Hall had considered Meillet's statements in full, he would probably have found him vindicated.

I do wish, on the other hand, to take exception to several details in Mr. Hall's argument. First, I do not see how it could be shown that "derivational patterns are more basic, closer to the core of linguistic structure, than are inflectional patterns" (p. 371). If a hierarchy obtains at all, I should think that it is the reverse, the evidence being that derivation is much more liable to be influenced by another language than inflection. Secondly, I do not feel that the fixed order, subject+predicate, is probative (no matter how "basic" its level) because the possibilities are so limited (S+P, P+S, or freedom) that chance as against influence or genetic inheritance can never be ruled out.[3] Finally, the separate references to phonology, vocabulary and grammar seem to me excessively loose. Phonology alone is, as Mr. Hall says, never a criterion of relationship, whereas vocabulary alone is not even a meaningful comparative concept. It would be clearest to state that THE criterion is the existence of cognates in the basic morpheme stock, with parallelism in allomorphic alternations as a powerful supplement. If we amend the definition of cognateness

[2] See John E. Reinecke, "Trade Jargons and Creole Dialects as Marginal Languages," *Social Forces* XVII (1938), 107-118.

[3] This, in fact, is the notorious weakness of Sten Konow's classification of the languages of Southeast Asia. Cf. Robert Shafer, "Classification of the Sino-Tibetan Languages," *Word* XI (1955), 94-111.

as "sound-meaning similarity"[4] to "similarity in expression (form) and in meaning or function," we can even dispense with the separate consideration of phonological correspondences in vocabulary and in grammar, since we then include under one heading segmental morphemes as well as non-segmental devices of expression, meaningful morphemes as well as the virtually "meaningless" functional morphemes of grammar.

4. Caribbean Convergence. When one first approaches the Creole languages, one has the impression that the basic vocabulary is of one origin (European) and the basic grammar of another ("African"). Now, in the well-known comparative fields, basic vocabulary and basic grammar have always been found to develop together. Theoretically, then, the Creoles might require us to set up a more elaborate typology of linguistic diversification, with grammar and basic vocabulary functioning as separate criteria for relationship, and with the Creoles representing an unusual case of overlapping criteria. The study of the Creoles would then indeed force an adjustment of linguistic theory.

But in what sense can we say that the basic grammar of the Creoles is non-Indo-European? We consider morphemes as cognate on the basis of correspondences of sound and meaning; for the purpose of grammar, we may even say: sound, meaning, and function. Now, if we take such grammatical morphemes of Haitian Creole as *ap-* (marker of progressive aspect) or *aw(a)-* (future marker), they seem to agree phonetically with French *après* and *avoir*, and represent but gradual modifications in meaning and function of these French words, just as English *gate* and German *Gasse* allow the reconstruction of gradual phonetic and semantic changes. The Creole morphemes thus satisfy the definition of cognates with respect to French. The same cannot be said of the relation of *ap-* and *aw(a)-* to, let us say, Ibo *na-* and *ga-*, where a rough similarity of meaning and function is detectable, but not a similarity of phonemic shape.

What the Creoles seem to manifest is extensive loan translation, or calquing, in their grammars. The calquing is perhaps more thorough than what has been encountered in other cases of language contact, but it is of the same type. Yiddish, for example,

[4] Joseph H. Greenberg, "Genetic Relationship Among Languages," in his *Essays in Linguistics*, Chicago, 1957, pp. 35-45. This chapter is probably the clearest statement of the problem on record.

distinguishes *geshribn* vs. *on-geshribn* ('was writing' vs. 'wrote') and *a briv-l* ('a note') vs. *a briv-ele* ('a small note [endearing]'), using Germanic-origin morphemes to form a perfective aspect and a second-degree diminutive on a Slavic model (cf. Polish *pisał—na-pisał*, *list-ek—list-eczek*). But the genetic relationship of Yiddish is with Germanic despite these un-German features of its grammar, because the morphemes in question are cognate (i.e. correspond in sound and meaning, as modified) with morphemes in Germanic, not in Slavic languages.

The relation of Creoles and pidgins to their stock is special not because they have broken away from their families—the theory of families is not so weak as to exclude their deviating children—but because they have undergone much calquing in their grammar. "If artificial languages such as Esperanto, pidgins, and Creoles are not to be ... distinguished from 'other' members of established families," Mr. Taylor writes in a letter of May 28, 1958, "...anthropologists [will] have to drop or change the inferences that they have been in the habit of drawing from inclusion within them." But in the light of the many previous warnings which anthropology has received about drawing extra-linguistic inferences from linguistic data, one would be surprised if there were still particular ethnological theories left to be dropped because of the present arguments about classification.

I agree with Messrs. Taylor and Hall that a term for the special relation of the Creoles to each other would be useful. But I do not feel, as does Mr. Taylor, that this relation affects the genetic classification of the languages, and I do not agree that this relationship should be called "'basic' or 'lexical'" (Taylor, p. 411) or "pidginetic" (Taylor, personal communication), or another term (Hall, p. 372). Linguists have discussed this relationship before, viz. under the heading of "Sprachbund" (Roman Jakobson).[5] It is the relation which exists, for example, among the Balkan languages or which describes the similarities of the vowel systems of Ukrainian, Southeast Yiddish, Rumanian, and Bulgarian, the consonantal similarities of the languages extending from Poland to Japan, or the use of classifiers in South and Southeast Asia.

Yet although the phenomenon is familiar, the term "Sprachbund" is admittedly unsatisfactory. Its fundamental

[5] The best treatment now is Henrik Becker in, *Der Sprachbund*, Berlin and Leipsic, 1948. On parallel ideas of Franz Boas, see Roman Jakobson, "Franz Boas' Approach to Language," *International Journal of American Linguistics*, X (1944), 188-195, p. 193.

fault is that it implies a unit, as if a language either were or were not a member of a given Sprachbund. But of course a grouping of this sort has no specific *a priori* criteria; a group of geographically continuous languages may be classified as a Sprachbund ad hoc, with respect to any structural isogloss. The proposed English equivalent of the term, "affinity," has the further disadvantage of implying not a class of languages, but its defining property. It would be preferable, I feel, to abandon these terms and to speak simply of cases of convergent development and, if necessary, of convergence areas.[6] We would then say that in the Caribbean area, as for example in the Balkans, a number of (genetically) Indo-European languages have undergone intense convergent development. In the formation of the Creoles, an unknown African substratum, mutual interinfluence, and the "minimization" (or "optimization") of grammar[7] which seems to be universally attendant upon improvised communication conditions, all played a role. Such a formulation challenges the integrity neither of Meillet nor of Indo-European.

[6] "Convergence area" is perhaps also more specifically meaningful than M. B. Emenau's "linguistic area" (*Language*, XXXII [1956], p. 16).

[7] See Uriel Weinreich, *Languages in Contact*, New York, 1953, p. 43, footnote 75. See also A. B. Dolgopol'skij, "Protiv ošybočnoj koncepcii 'gibridnyx' jazykov (o kreol'skix narečijax)," *Grammatičeskij stroj* (=Učenye zapiski 1-go Mosk. gosud. pedag. instituta inostrannyx jazykov, VII), Moscow, 1955, 29-60, esp. p. 41.

REVIEWS

ROBERT GODEL, *Les sources manuscrites du* Cours de linguistique générale *de F. de Saussure*. Société de publications Romanes et Françaises sous la direction de Mario Roques, LXI. 283 pp. Genève et Paris : Librairie E. Droz et Librairie Minard, 1957.

L'année du centenaire de la naissance de Saussure a été marquée par deux publications importantes de Robert Godel ayant trait à la linguistique saussurienne. La première est l'édition de l'« Introduction » du deuxième cours de linguistique générale professé par Saussure à l'Université de Genève (cf. ci-dessous mon compte rendu de *CFS* 15) ; la seconde, le livre faisant l'objet de la présente analyse. Le but de cet ouvrage, comme l'auteur le déclare dans l'Avant-propos, est de confronter le *Cours de linguistique générale (CLG)* avec les manuscrits dont se sont servis les éditeurs pour réaliser leur tâche. Godel s'applique pour ceci, d'abord, à déterminer les sources précises du plan et du texte du *CLG*. Il reprend ensuite, à partir des manuscrits, les problèmes d'interprétation que les éditeurs ont eu à résoudre. A cette division du travail correspondent les deux chapitres fondamentaux du livre : le chapitre III, « Le travail des éditeurs », et le chapitre IV, « Problèmes d'interprétation ». La rédaction de ces chapitres n'était cependant possible qu'après avoir produit « toutes les pièces du dossier », c'est-à-dire, qu'après avoir fait d'une façon ou d'une autre connaître au lecteur les manuscrits utilisés par Bally et Sechehaye. Godel résout le problème qui se posait à lui (ces manuscrits occupent plus de mille deux cents pages) en donnant un résumé analytique qui constitue le chapitre II du livre. Le chapitre I traite, en manière d'introduction, de « La place de la linguistique générale dans la vie de F. de Saussure ». Enfin, Godel donne en appendice un lexique de la terminologie saussurienne qui s'ajoutera heureusement à la série de lexiques similaires qu'a commencé à publier le C. I. P. L. Tel est le plan, hardi et logique à la fois, de l'ouvrage.

Le chapitre sur « Le travail des éditeurs » traite de trois sujets principaux. Tout d'abord Godel détermine les fondements sur lesquels les éditeurs se sont appuyés pour établir le plan du *CLG;* puis il cherche à retrouver la source manuscrite précise de chaque chapitre, paragraphe ou alinéa du texte publié par Bally et Sechehaye ; la confrontation de ce texte avec ses sources permet enfin à l'auteur d'apprécier dans quelle mesure les éditeurs se sont écartés de celles-ci en y introduisant des retouches de terminologie, de style, etc. Deux listes insérées dans ce chapitre seront particulièrement appréciées. La première, « Détail des sources du *CLG* », reproduit la table de matières du *CLG* avec indication des sources de chaque paragraphe au moyen de renvois aux textes analysés dans le chapitre II. La seconde est une analyse des passages où Bally et Sechehaye ont jugé bon d'expliciter ou de compléter le texte manuscrit. Godel indique dans chaque cas en quoi consiste l'initiative des éditeurs et se prononce parfois sur son opportunité. Il cite, le cas échéant, les textes qui pourraient justifier leur point de vue, ou au contraire, s'y opposer. Le chapitre se termine par un « Spécimen d'édition critique » où les §§ 2 et 3 du chapitre du *CLG* sur la nature du signe linguistique (chap. I de la première partie) servent à montrer ce que serait une édition critique du livre de Saussure.

Dans le chapitre sur « Les problèmes d'interprétation » Godel s'occupe seulement de ce qui a trait aux divisions générales de la linguistique et à la linguistique statique, savoir : « La question des identités », « Langue et Parole », « La linguistique et ses subdivisions » et « Les entités de la langue ». La question de la distinction langue-parole est considérée sous l'aspect général, puis sous deux aspects particuliers : « Le phonème » et « La phrase et le syntagme » ; le paragraphe sur « Les entités de la langue » se subdivise en cinq parties, chacune traitant d'un aspect partiel du problème : « Le signe et ses caractères », « Le principe de linéarité », « La question des unités », « Les termes et le système » et « La langue, système de valeurs ». La simple énonciation de ces titres suffit à montrer l'importance des questions que Godel a choisies afin de réinterpréter, à partir des manuscrits, les idées de Saussure à leur propos. La méthode suivie a consisté fondamentalement à confronter les passages parallèles des manuscrits pour relever les concordances et apprécier l'importance et la signification des divergences. L'auteur cherche ainsi à saisir la pensée du maître, non pas « sous sa forme définitive », comme ont voulu le faire Bally et Sechehaye, « mais autant qu'il est possible, dans son deve-

nir, dans ses hésitations, ses variations peut-être ». Godel a raison lorsqu'il affirme qu'en agissant ainsi on ne fait outrage ni à la mémoire des éditeurs ni à celle de Saussure. Quant aux éditeurs, il est incontestable qu'en s'efforçant à présenter « sous leur forme définitive » les idées de Saussure, ils ont fait ce qu'il fallait à l'époque pour qu'elles contribuassent le plus au progrès de la linguistique. La pensée de Saussure en est sans doute un peu défigurée — au moins dans ce qu'elle avait de dynamique —, mais la tâche qui s'imposait il y a quarante ans était avant tout de faire comprendre aux linguistes l'importance exceptionnelle de l'enseignement du maître. Ce but aurait été très difficilement atteint en publiant sans modification des notes d'étudiants. Quant à Saussure, la nouvelle présentation de sa pensée par Godel, loin d'être un outrage à sa mémoire, constitue à mon sens l'acquittement d'une dette qu'on avait envers lui. Si en 1916 on a dû présenter sa pensée sous un aspect qui n'était pas tout à fait le sien, il fallait que finalement elle nous fût rendue telle que les étudiants l'avaient reçue : avec ses hésitations et ses variations certes, mais par là aussi dans toute sa puissance originelle. C'est ce que fait Godel dans le chapitre IV de son livre, et la figure intellectuelle de Saussure n'en sort qu'agrandie : dans le *CLG* on n'apercevait qu'à moitié l'extraordinaire intuition des problèmes qui était celle de Saussure, l'ampleur avec laquelle il les embrassait tenant compte de toutes leurs connexions.

En éclairant d'un jour nouveau un des épisodes les plus complexes et les plus riches en conséquences de l'histoire de la linguistique : l'enseignement de Saussure et la diffusion posthume de ses idées par deux de ses disciples, l'ouvrage de Godel est de la plus grande importance sur le plan historique. Mais ce n'est pas là le seul mérite du livre. Un des résultats du travail de Godel est en effet de montrer qu'il restait encore dans les manuscrits où a été enregistrée la pensée saussurienne des éléments dont on peut tirer profit actuellement pour la solution des problèmes. Il apparaît surtout que Saussure s'était parfois mieux posé certains problèmes, avait mieux tenu compte de leurs ramifications qu'on ne le fait aujourd'hui. C'est pourquoi la lecture du livre de Godel apportera certainement de nombreuses suggestions au linguiste préoccupé des questions théoriques. Celles qui, par ma part, me sont venues à l'esprit, ne sauraient s'insérer dans ce compte rendu et feront l'objet d'un article postérieur. Je voudrais cependant illustrer ici mes appréciations. Godel montre (pp. 224 et ss.), par exemple, que Saussure attribue à la langue la qualité de « système » pour

deux raisons bien différentes : l'une, c'est le non isolement du signe : « il n'y a pas de signes isolés » ; l'autre, c'est le parallélisme des oppositions et la limitation de l'arbitraire qui s'en suit : « toute langue [...] forme un système [...]. C'est le côté par où elle n'est pas complètement arbitraire ». Or il me semble qu'entre ces deux faits qui, d'après Saussure, font de la langue un système, il faille établir une hiérarchie. Le non isolement du signe est en effet un principe sémiologique fondamental, valable pour tout signe, linguistique ou non (et partant, malgré Saussure, indépendant de la prétendue « continuité du champ sémantique » de la langue). Un signe ne signifie quelque chose et n'est donc un signe que par rapport à d'autres signes qui pourraient être employés à sa place ; ceci peut être dit aussi bien d'un mot que du feu rouge qui ordonne l'arrêt au croisement de deux rues. Si l'on retient comme notions de rapport associatif et de rapport syntagmatique celles qui ressortent de la comparaison d'un mot avec un élément d'un édifice comme par exemple une colonne (*CLG*$_2$, p. 171), on dira qu'un signe n'existe qu'en rapport associatif avec d'autres signes ou encore qu'un signe n'existe que comme terme d'une série associative. Le parallélisme des oppositions ou, plutôt, des rapports associatifs, n'est par contre qu'une caractéristique secondaire au point de vue sémiologique : elle n'est nullement indispensable, et n'est qu'un moyen pour réduire le domaine de l'arbitraire dans les systèmes d'extension considérable. Cette caractéristique n'apparaît pas, par exemple, dans le système « feu rouge » — « feu orange » — « feu vert ».

Or ce n'est que ce parallélisme des rapports associatifs, par lequel on arrive à la limitation de l'arbitraire, qui permet l'analyse des signes en signes plus simples, et ce n'est qu'entre ces signes-ci que s'établissent les rapports de type syntagmatique. La hiérarchie des faits sur lesquels Saussure se fonde pour attribuer un caractère systématique à la langue implique la hiérarchie des rapports qui unissent les signes : les rapports syntagmatiques sont subordonnés aux rapports associatifs. Ceux-là, en effet, ne sont que la conséquence d'un agencement particulier de ceux-ci. On se demande alors si on a le droit de rejeter la phrase ou, mieux, l'énoncé, dans la « parole », ou si, bien au contraire, on ne doit pas le considérer comme l'unité de base de la langue. En effet, c'est avant tout entre les énoncés que s'établissent les rapports essentiels à tout système sémiologique, les rapports associatifs dont dépend la valeur des signes. Toutes les autres unités, celles qui sont terme des rapports syntagmatiques comme le mot ou le monème, n'existent qu'à

l'intérieur d'une unité d'ordre supérieur et du fait du parallélisme des rapports associatifs entre cette unité-ci et d'autres du même ordre. C'est-à-dire, qu'en définitive les unités telles que le mot ou le monème n'existent qu'à l'intérieur de l'énoncé et du fait du parallélisme des rapports associatifs entre énoncés. Ainsi, si dans une langue toute « motivation relative » disparaissait, il ne resterait dans cette langue hypothétique d'autre unité que l'énoncé : tant qu'il y aurait, par exemple, des mots, il est évident que la « motivation relative » n'aurait pas tout à fait disparu.

L'enseignement de Saussure est un événement qui ne termine pas une période de l'histoire de notre science, mais qui en commence une. Il n'est pas le terme, mais le point de départ d'un courant de pensée particulièrement vivant aujourd'hui. Le grand mérite de Bally et Sechehaye a été de mettre en pleine lumière cet événement capital et de le sauver ainsi de l'oubli. Celui de Godel est de lui rendre toute son ampleur. Godel, avec son livre, a complété dignement l'œuvre de ses maîtres.

Paris, C. N. R. S. L. J. PRIETO

Cahiers Ferdinand de Saussure, 15. 138 pp. Genève : Librairie E. Droz, 1957. 16 francs suisses.

Le fascicule des *Cahiers Ferdinand de Saussure* correspondant à l'année 1957 contient trois contributions.

Robert GODEL publie (pp. 3-103) l'« Introduction » d'un des cours de linguistique générale professés par Saussure, telle qu'elle a été notée par ses étudiants. On sait que Saussure donna trois cours de linguistique générale, tous trois à l'Université de Genève, et que les notes prises alors par les auditeurs ont été la principale source utilisée par Bally et Sechehaye pour l'élaboration du *Cours de linguistique générale*. L'« Introduction » publiée par Godel correspond au deuxième de ces cours, donné pendant l'année 1908-1909. Le texte a été fourni principalement par les cahiers d'A. Riedlinger, qui se sont révélés les plus complets, mais les notes de deux autres auditeurs, F. Bouchardy et L. Gautier, ont été utilisées aussi, et, lorsqu'elles présentaient des passages qui manquaient dans la version de Riedlinger, ces passages ont été insérés à leur place sûre ou probable. D'autre part, dans certains cas, lorsque la version de Bouchardy ou celle de Gautier paraissait plus satisfaisante, elle a remplacé dans le texte la version de Riedlinger, donnée alors en note. L'édition de ce texte par Godel continue l'œuvre commencée

il y a quatre ans avec les *Notes inédites de F. de Saussure* (*CFS* 12, pp. 49-71). L'intérêt de la publication des manuscrits utilisés par Bally et Sechehaye vient sans doute de ce qu'elle permet une connaissance plus directe de la pensée saussurienne, mais aussi de ce qu'elle est indispensable à une édition critique du *Cours*. C'est ce que nous explique Godel dans un livre récent dont nous rendons compte ci-dessus (*Les sources...*, p. 102). Souhaitons qu'après la publication de ces manuscrits une édition critique du livre de Saussure couronne l'œuvre de Godel. Nul en effet n'est mieux préparé que lui pour une telle tâche.

La seconde contribution est signée d'André MARTINET et a pour titre : « Arbitraire linguistique et double articulation » (pp. 105-116). « Chaque réalisation d'un phonème donné, dans un mot particulier, reste solidaire des autres réalisations du même phonème dans tout autre mot » (p. 110). Martinet avait apporté, dans son *Économie des changements phonétiques*, une preuve de ce fait qui assure la réalité de ce qu'on appelle la « seconde articulation » : le besoin de maintenir distincts les signifiants quasi-homonymes explique parfois la modification de la réalisation d'un phonème (la modification du « champ de dispersion » d'un phonème) ; mais cette modification se produit non seulement dans les signifiants qui, sans elle, risqueraient de se confondre avec un autre, mais aussi dans les signifiants où il n'y a aucun danger de confusion (cf. Saussure, *Cours*$_2$, p. 133). Dans le présent article l'auteur signale une conséquence de l'articulation du signifiant en phonèmes, conséquence qui s'accorde également bien avec la pensée saussurienne : la solidarité des réalisations d'un phonème dans tous les mots où il apparaît assure l'autonomie du signifiant vis-à-vis du signifié et, par là, l'arbitraire du signe. Si les signifiants s'opposaient en bloc les uns aux autres, c'est-à-dire, s'ils étaient inarticulés, rien n'empêcherait les locuteurs, tant que les signifiants se maintiendraient distincts, de les modifier de façon qu'ils s'adaptent le mieux à la notion à exprimer. L'arbitraire du signe en serait vite immolé à l'expressivité.

La dernière contribution est un article de de Rulon WELLS, «A Mathematical Approach to Meaning» (pp. 117-136), où l'auteur présente et discute les idées de base de la méthode de L. L. Thurstone dite de la « multiple-factor analysis », telle que C. L. Osgood l'a appliquée au «meaning».

Paris, C. N. R. S. L. J. PRIETO.

HARRY A. DEFERRARI, *The Phonology of Italian, Spanish and French.* Washington D. C., 1954.

Alors que les premières décades du structuralisme ont vu les linguistes d'avant-garde se consacrer entièrement à des études synchroniques, il semble que depuis quelques années ce genre de recherches est arrivé à un point de saturation et que, un peu partout, naissent maintenant des préoccupations diachroniques. La linguistique historique traditionnelle se bornait à la constatation du changement, à l'établissement de correspondances, et, dans ses tentatives d'explication, elle ne retenait que des causalités externes, car elle manquait des outils nécessaires à une explication purement linguistique. Ce sont ces outils qui sont maintenant à la disposition des linguistes, qu'ils se réclament ou non du structuralisme.

C'est le milieu du structuralisme européen, et plus particulièrement de l'école dite pragoise, qui s'est révélé le plus propice à la naissance d'une théorie diachronique, comme le montre l'*Économie* de Martinet. Le structuralisme américain (bloomfieldien) au contraire s'enferme davantage dans la synchronie et c'est par réaction contre lui que se forme la théorie que propose Harry A. Deferrari dans l'introduction de son livre posthume *The Phonology of Italian, Spanish and French.*

Il est intéressant de constater que ces deux théories, apparues indépendamment l'une de l'autre, se fondent sur une même analyse de la réalité linguistique ultime.

Martinet et Deferrari s'opposent par leur formation : le premier, structuraliste avoué et spécialiste, non d'un certain groupe de langues, mais de linguistique diachronique en général ; le second, romaniste de formation philologique traditionnelle. Mais ils s'apparentent par leur réalisme en s'opposant l'un au structuralisme glossématique, l'autre à la linguistique bloomfieldienne. En effet, les deux livres commencent par une critique du formalisme de certains structuralistes : « quiconque postule pour le phonème une existence psychique est un « réaliste » au même titre que celui qui attribue au phonème une réalité neuro-musculaire. Les formalistes conséquents ne voient dans toute unité linguistique, qu'un faisceau de relations. Les réalités physiques, musculaires ou psychiques correspondantes ne les intéressent pas ». Or, dans la description synchronique qui est surtout organisation des faits, classement des relations entre les éléments du système, le formalisme peut être une réussite, mais en diachronie, si on néglige les faits marginaux, les variations non pertinentes, on aboutit à l'impasse.

L'efficacité d'une théorie diachronique se fonde sur la prise de conscience de la complexité de la réalité phonétique : à un moment donné d'une langue donnée, les différents membres d'une même communauté linguistique donnent à un phonème donné des réalisations différentes. C'est ici que Martinet introduit la notion de « champ de dispersion » : « Nous devons tenir compte de la possibilité d'un « champ de dispersion, même dans dans le parler d'une seule personne, et à plus forte raison sans doute si nous considérons tous les membres d'une communauté. L'existence de ce champ de dispersion ne fait pas de doute lorsqu'on considère un phonème dont les variations combinatoires sont importantes, c'est-à-dire une unité phonologique dont la réalisation varie largement selon le contexte ». Cette indifférence aux variations non-phonématiques (allophones) est le reproche majeur que fait Deferrari au phonématisme tel qu'il a été compris en Amérique (et pour lequel il prédit la faillite la plus complète à brève échéance), tout en reconnaissant que la théorie du phonème aurait pu aboutir à des résultats différents. Cette complexité synchronique reflète un aspect de l'évolution, la « sélection tâtonnante » (trial and error process) : à un problème donné, la langue répond par plusieurs tentatives de solution dont normalement une seule est couronnée de succès dans un dialecte donné.

Deferrari est américain cependant par sa définition du phonème : « a group of closely related sounds centering around the most frequently used member of the group », et s'oppose par là à Martinet: « un phonème peut être considéré comme un ensemble de traits pertinents qui se réalisent simultanément ». Cette différence est de conséquence pour la théorie diachronique : en effet, tandis que Martinet peut opérer au niveau du trait pertinent (qu'il définit comme unité de changement) ou non-pertinent, Deferrari doit s'en tenir au niveau du phonème et de ses allophones.

Martinet et Deferrari ont tous les deux conscience que les tentatives d'explication diachronique qui les ont précédés ont échoué par excès d'ambition : elles ne réussissent pas à réaliser leur prétention d'être complètes.

Les deux théories dont il s'agit ici n'ont pas l'intention de tout expliquer, mais seulement de tirer le maximum des données purement linguistiques. " Until much more research has been done, it is granted we do not have absolute proof of the basic reasons for sound changes. However we should make after-the-fact analyses in order to see if we may find at least partial " explanations " of the sound changes in terms of the effect of

surrounding sounds and terms of observable linguistic "tendencies" which seem to characterize a given language or related group of languages". A la différence de la théorie de l'hérédité ou de la théorie des strats qui met en jeu plusieurs structures et leurs interférences, ces deux linguistes ne veulent s'occuper que du conditionnement constant des changements linguistiques à l'intérieur d'une même structure. Il est opportun d'éclairer ici la distinction entre facteurs externes et facteurs internes : « On opposera ce qui, dans une communauté donnée, fait nécessairement partie du courant qui s'établit entre les interlocuteurs, à ce qui n'intervient dans ce courant qu'à titre accidentel ». Les changements dits « conditionnés » font partie de la causalité interne au même titre que les changements « spontanés » puisque la réalisation phonétique intervient nécessairement dans le circuit de la communication.

La cause fondamentale des changements linguistiques est dans la nature même du langage : l'antinomie de l'inertie (la tendance au moindre effort) et des nécessités de la communication : "Man has perceived unconsciously (and, only to a slight degree, consciously) that it is well for him to make his speech as efficient as it may be necessary to do the things that speech can do, but that energy (and time) are needed for other vital tasks. Consequently, every language that has ever been used in the history of the world has always strived to do whatever language could do (in its time and circumstances) and it has always strived to do its task with a minimum expenditure of effort", et Martinet : « L'évolution linguistique en général peut être conçue comme régie par l'antinomie permanente des besoins communicatifs et expressifs de l'homme et de sa tendance à réduire au minimum son activité physique et mentale ». En vertu de ce principe du moindre effort (« économie », dans la terminologie de Martinet), si on part d'un état synchronique donné d'un dialecte donné, les locuteurs tendent à le simplifier au cours des âges : c'est-à-dire que, dans la perspective adoptée par Deferrari, les sons voisins dans la chaîne parlée tendront à s'assimiler les uns aux autres, et dans celle de Martinet, que les oppositions dont le rendement fonctionnel est faible tendent à disparaître et les phonèmes non-intégrés ou faiblement intégrés, à s'intégrer davantage. Cependant, pour que la compréhension soit possible, il ne faut pas que cette tendance soit livrée à elle-même : elle est contrôlée par la nécessité de l'efficacité. Il ne faut d'ailleurs pas prendre cela au sens strict, c'est-à-dire où un changement qui risquerait de provoquer des homonymies ne peut pas se produire. En effet l'homonymie n'est qu'assez rarement gênante

(seulement entre les mots d'une même classe) et la langue dispose de moyens pour y remédier : dérivation, composition, substitution, etc.

On pourrait objecter que, par ce processus, on devrait arriver à un moment donné à une situation optimum où un maximum de simplicité permet une compréhension intégrale. Mais (et c'est ce que souligne Martinet dans le passage cité plus haut) il faut compter avec une fonction secondaire du language, la fonction expressive : on peut constater que les besoins expressifs d'une communauté donnée changent avec le temps entraînant des déséquilibres et par conséquent déchaînant des changements dans les structures.

Les deux théories s'appliquent aux changements conditionnés aussi bien qu'aux changements spontanés (ceux qui ne sont pas causés par le contexte de la chaîne). Cependant on est obligé de reconnaître que ces termes traditionnels n'ont pas de valeur véritable dans le cadre de ces théories. En effet, tout en admettant la plausibilité d'autres explications, Deferrari s'efforce de ramener les changements spontanés à des changements conditionnés : il essaie p. ex. d'expliquer le passage de [r] à [ʀ] en français par la vélarisation de [r] lorsqu'il se trouvait en contact avec des voyelles d'arrière ; en vertu de la vitesse acquise, ce *r* vélaire serait devenu uvulaire (momentum theory : il arrive qu'un mouvement une fois commencé se prolonge au-delà de ce qu'exigaient les besoins de la communication). Une autre possibilité serait l'affaiblissement de *r* et le risque d'assimilation aux articulations d'avant. Par réaction (flight from assimilation) l'articulation de *r* devient vélaire et finalement uvulaire. En ce qui concerne la théorie de Martinet, l'expression « changement spontané » est tout à fait hors de mise, puisque cette notion présuppose l'existence d'un choc initial et que Martinet se propose précisément de prouver que ce choc n'est pas nécessaire, mais qu'il existe à l'intérieur des systèmes un conditionnement permanent des changements.

Les explications diachroniques de Deferrari se veulent fondées uniquement sur le contexte phonétique, cependant il est constamment obligé de recourir plus ou moins consciemment à des notions que Martinet a dégagées de façon nette et lucide et qui ont trait non pas à la chaîne mais au système : fonction, structure, économie.

En effet il reconnaît que l'environnement phonétique n'est pas le seul facteur de changement : " A changing sound from a source of relatively frequent occurrence tends to determine the phonetic course (i.e. the manner of phonetic development) which is followed by the same sound from a source of relatively less frequent occurr-

ence ». Sous la confusion et la maladresse de l'expression, il faut probablement entendre qu'il arrive que la variante la plus fréquente d'un phonème détermine l'évolution des variantes moins fréquentes: un changement causé par un certain contexte sur une variante donnée d'un phonème se produira également dans d'autres contextes qui, à première vue, ne sont pas propices au changement en question, par une sorte de sentiment de l'unité du phonème et pour préserver les relations entre phonèmes dans le système. C'est probablement à un phénomène de ce genre que Deferrari fait allusion dans l'exemple cité plus haut de [r] et [ʀ] en français.

Cette notion d'un système qui fait pression sur les solutions phonétiques que trouve la langue aux situations dans la chaîne se retrouve dans l'explication que donne Deferrari de la diphtongaison dans les langues romanes. Il faut se remettre en mémoire ce que dit Martinet de l'attraction (2.15), des chaînes de propulsion et de traction (2.28). Un phonème donné A se réalise dans un certain champ limité par les champs de dispersion des autres phonèmes. Si le champ de A se déplace, d'une part il se crée un vide et par conséquent le champ des phonèmes environnants perd une limite et il arrive souvent que l'un de ces phonèmes, attiré par le vide, vienne le remplir, et que se déclanche une réaction en chaîne, mais d'autre part le champ de A tend à empiéter sur celui d'un autre phonème B, c-à-d. à se confondre avec lui, et il arrive généralement que, pour conserver une opposition utile à la compréhension, le champ de B se déplace devant A. Ces deux dernières idées se retrouvent dans la façon dont Deferrari explique la diphtongaison de ɛ p. ex. : en italien et en espagnol le champ de ɛ tendait à se confondre avec celui de *e*. Afin d'éviter des confusions, ɛ s'est diphtongué, c'est-à-dire que le champ de ses réalisations s'est déplacé. En français le champ de ɛ se déplaçait vers *e* et celui de *e* vers *i* ; pour conserver les oppositions, on aurait eu des diphtongaisons aux deux degrés moyens. Ces diphtongaisons ne se produisent pas au hasard, mais compte tenu de la position relative des phonèmes dans le système : prenant comme point de départ l'ellipse vocalique de Jones, on admet que les changements qui exigent le minimum d'énergie (c'est-à-dire le trajet le plus court sur l'ellipse) sont les plus indiqués et que les changements qui pourraient amener des confusions avec d'autres phonèmes sont systématiquement évités. " We assume, in linguistic change, that intervening sounds which are not phonemic sounds in the language at the time involved offer no obstacle or hinderance to the change. We likewise assume that every intervening phonemic sound presents an obstacle or hinderance. "

Les notions de « série », d'« ordre » et de « corrélation » (« On dira que des phonèmes consonantiques caractérisés par une même articulation forment une série si leurs autres articulations caractéristiques sont situées en différents points le long du chenal expiratoire ; on dira de phonèmes caractérisés par une même articulation située en un point donné du chenal expiratoire, mais distingués l'un de l'autre par quelque autre articulation distinctive, qu'ils forment un ordre ; série et ordre présupposent une unité plus grande les groupant en un tout, à savoir la corrélation, qui comprend deux séries parallèles et un certain nombre de phonèmes accouplés appartenant aux mêmes ordres ») n'apparaissent pas clairement définies chez Deferrari, mais il faut les sous-entendre dans les généralités qu'il exprime au début de chaque section, où l'évolution de chaque son n'est pas présentée isolément, mais où il essaie de grouper ses remarques et de dégager ce que le développement de certains sons peut avoir de commun. Il est fâcheux que Deferrari par souci de se conformer à la tradition romanistique et pour ne pas choquer le public auquel il s'adresse n'ait pas ouvertement appliqué sa théorie et dégagé toutes ses implications dans son livre.

La notion de système comme cadre de l'évolution phonétique se retrouve également dans ce qui a trait aux emprunts : en espagnol p. ex. les consonnes géminées ont été assez tôt éliminées ; il a fallu trouver une solution au problème créé par les mots à géminées introduits plus tard dans la langue et qui ne pouvaient pas s'intégrer tels quels dans le système : pour *ll* et *nn* p. ex. la solution est *ld* et *nd*, cf. Sp. 242, 247.

On voit que dans les implications de la théorie de Deferrari on retrouve à l'état embryonnaire les notions centrales de celle de Martinet. Les deux théories apparaissent comme complémentaires et en tout cas témoignent de l'actualité des problèmes diachroniques et des solutions convergentes qui peuvent leur être données des deux côtés de l'Atlantique.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

AMADO ALONSO, *De la pronunciación medieval a la moderna en español*, ultimado y dispuesto para la imprenta por Rafael Lapesa. Tomo primero, 452 pp. Madrid: Editorial Gredos, 1955.

Une mort prématurée a empêché Amado Alonso de terminer

l'ouvrage qui devait couronner ses recherches sur la phonétique et la phonologie historiques de l'espagnol. Rafael Lapesa a assumé la tâche ardue de le compléter et de le publier. Il s'agit d'une étude des changements qui ont fait du système phonologique de l'espagnol médiéval le système de l'espagnol moderne. Cette étude est fondée principalement sur les considérations que les grammairiens espagnols et étrangers ont émises, à propos de la prononciation espagnole, à l'époque où les changements en question étaient en train de se produire. Le premier volume, seul paru des trois devant constituer l'ouvrage complet, comporte une brève introduction et trois chapitres : sur *b* et *v*, sur *d* et sur *c* et *z*.

L'auteur pense les phénomènes étudiés dans un cadre fonctionnel : il se souvient toujours que les sons sont les réalisations des phonèmes ; il tient soigneusement compte des conséquences qui découlent, pour la distinction des phonèmes, des changements survenus dans leurs réalisations. Ceci ne veut pas dire cependant que les linguistes « traditionalistes », qui ne sont pas au courant des théories développées ces vingt dernières années, se sentiront mal à l'aise en lisant ce livre. En effet, les termes techniques propres à une école déterminée y sont pratiquement absents et, par ailleurs, la dose de « fonctionalisme » n'est pas plus grande ici qu'elle peut l'être, en fait, dans n'importe quel traité de phonétique historique ; la différence est qu'Alonso l'introduit de façon consciente et conséquente.

Ce seront les linguistes ayant suivi de près l'évolution des théories linguistiques depuis la fin de la dernière guerre qui trouveront parfois discutables certaines formulations d'Alonso. Ainsi, aucun phonologue ne serait, me semble-t-il, d'accord pour affirmer, à propos de la confusion de deux phonèmes, que « ce fut l'évolution phonétique, avec l'usure des articulations qu'elle provoque, qui, sans doute, a bouleversé la conscience phonématique et la volonté de distinction » (p. 390). De même, on lit à la page 19 que les changements phonétiques se produisent « dans chaque langue suivant sa propre tradition, suivant les goûts et préférences collectives et durables » ; l'auteur explique donc la diversité des évolutions phonétiques dans les différentes langues par des notions aussi peu susceptibles d'utilisation scientifique que le « goût collectif », et ne dit rien par contre du seul facteur permanent qu'on ait constaté jusqu'à présent : la pression réciproque des composants de la chaîne entre eux et des composants du système entre eux. On ne saurait cependant exagérer l'importance de passages comme ceux que je viens de citer. Alonso a voulu faire avant tout une

étude descriptive, non explicative, de l'évolution de l'espagnol pendant une période déterminée. A ce titre, son livre est fondamental et désormais indispensable, ne laissant rien à désirer même au théoricien le plus soucieux de rigueur. L'auteur ne présente des points de vue dépassés qui donnent prise à des objections que dans les passages où il cherche à dégager les causes des changements phonétiques. Ce sont, comme on le voit, des passages secondaires, en marge du sujet principal. La valeur du livre n'en est pratiquement pas diminuée, et on ne saurait reprocher à l'auteur de ne pas avoir revu ses idées en la matière, puisqu'une linguistique explicative digne de ce nom n'est que tout à fait récente.

Il est deux détails, qu'on peut appeler de présentation car ils n'affectent pas la doctrine du livre, sur lesquels je voudrrais insister particulièrement dans l'espoir de les voir corrigés dans les volumes à venir. Il s'agit tout d'abord de l'emploi du terme « signe » au lieu de « phonème ». On nous dit par exemple, à la page 389, que « la différence de sonorité était la seule qui comptait pour le sentiment linguistique des espagnols [...] pour [...] sentir *c* et *z* comme deux *signes* différents » (je souligne). Rien ne justifie cet emploi du terme « signe » : il manque de tout appui dans la tradition et crée un conflit avec l'emploi consacré du terme ; puisqu' on sait qu'il est fréquent d'opposer les termes « signe » et « phonème » comme désignations de deux unités linguistiques radicalement différentes.

Je voudrais enfin attirer l'attention de l'éditeur sur la transcription. En général, les italiques sont employées dans l'ouvrage pour les exemples donnés en écriture (orthographique, comme *Madrid*, ou non, comme *Madriz* pour le castillan moderne), ainsi que pour la transcription phonétique et pour la transcription phonologique, si bien que pour certains passages le lecteur doit deviner ce que l'auteur veut dire. Mais ce qui est surtout critiquable est que la transcription *phonétique* est parfois donnée entre barres obliques. Or, c'est pour la transcription *phonologique*, et précisément pour la distinguer de la transcription phonétique, qu'on a commencé à utiliser systématiquement les barres obliques ; cet emploi a été généralement respecté jusqu'à présent. C'est, me semble-t-il, contribuer efficacement à étendre l'anarchie terminologique dont pâtit actuellement la linguistique que de méconnaître les quelques normes qui jouissent d'une acceptation plus ou moins générale.

Paris, C. N. R. S. L. J. PRIETO

TOMÁS NAVARRO, *Documentos lingüísticos del Alto Aragón*. IX+ 231 pp. Syracuse (New York) : Syracuse University Press, 1957.

Pour faciliter les études sur la chronologie et sur la distribution géographique des dialectes romans de la Péninsule à l'époque primitive, le Centro de Estudios Históricos de Madrid entreprit en 1919 la publication d'une collection d'actes notariaux d'intérêt linguistique. Cette collection devait se composer de trois parties. En 1936, au début de la guerre civile qui entraîna la dispersion des principaux collaborateurs du Centro, seule la première partie était parue. Elle comprenait des documents de l'ancien royaume de Castille édités par R. Menéndez Pidal[1]. La deuxième partie, où devaient être publiés des documents du royaume de Léon, n'a jamais dépassé, à ce qu'il semble, l'état de projet. La troisième partie enfin, consacrée à des documents des royaumes d'Aragon et de Navarre, était sous presse en 1936, mais on n'en avait plus parlé après la guerre. Une partie des documents destinés à cette troisième partie paraît maintenant dans la série de *Publicaciones del Centro de Estudios Hispánicos* de l'Université de Syracuse. Il s'agit des 150 documents qui devaient représenter le Haut Aragon ; ils proviennent de la province de Huesca et de la partie de la province de Saragosse située entre le Huesca et la Navarre (Las Cinco Villas). La ville de Huesca et sa circonscription sont représentées par 68 documents, dont le plus ancien est daté en 1258 et le plus récent en 1369 ; 40 documents, datés entre 1266 et 1476, proviennent de Jaca et sa circonscription ; 24 de la circonscription de Boltana (1263-1495) ; 9 de la circonscription de Barbastro (1279-1445) ; 2 de la circonscription de Benaberre (1338-1360) ; 1, daté en 1276, du village d'Alcubierre, circonscription de Sariñena, et 6 enfin de la zone de Las Cinco Villas, leurs dates se situant entre 1266 et 1301.

Les romanistes sauront gré à Navarro de cette publication, qui met à leur disposition des matériaux très intéressants édités avec la plus rigoureuse technique philologique. Mais ils apprendront avec regret que le reste de ce qui devait constituer la troisième partie des *Documentos lingüísticos de España*, recueilli dans le

[1] *Documentos lingüísticos de España. I, Reino de Castilla*, por Ramón Menéndez Pidal. Madrid, 1919.

Bas Aragon, dans l'ancien royaume de Navarre et dans le diocèse de Segorbe (province de Castellon), a été perdu pendant la guerre civile.

Paris, C. N. R. S. L. J. PRIETO.

MANUEL ÁLVAREZ NAZARIO, *El arcaismo vulgar en el español de Puerto Rico.* 219 pp. Mayagüez (Puerto Rico), 1957.

In this work, which was presented in 1954 as a doctoral dissertation at the University of Madrid, the author examines the speech of his fellow countrymen—particularly that of the island's 'jíbaro' or rustic inhabitants—for archaisms. A brief Introduction (17-18) is followed by six chapters dealing, respectively, with the concept and significance of linguistic archaisms (19-28), the reasons for their prevalence in Puerto Rico (29-37), phonetics (39-80), morphology (81-100), syntax (101-120) and lexicon (121-181); and by a Summary (183-186), a Bibliography (187-197) and an Index of words and phrases (199-219).

Dr. Álvarez recognizes that the term 'linguistic archaism' is vague and susceptible of different interpretations. For the purposes of this study he takes it to include: "aquellas formas de expresión desechadas por la lengua culta de Puerto Rico y por tanto ya olvidadas por completo o refugiadas en el habla de las capas populares de nuestra sociedad." This applies both to forms of Spanish ancestry and to such others as were borrowed from native or from African languages. On the other hand, spoken and written forms still current among educated Puerto Ricans, but which have fallen into disrepute or disuse in any or all other Spanish-speaking countries, are not here regarded as archaisms.

Retention of such archaisms as subsist among the peasants and urban working classes is attributed to the island's early Hispanicization, to its relative isolation from the mother country and to its lack of a strong cultural centre of its own during the colonial period. The vast majority of early Spanish settlers consisted of labourers and artisans, whose speech already contained features that were archaic among the educated classes of their own time and country; and Álvarez seems to take for granted "la tendencia de las clases populares a conservar aquellas formas de la lengua ya desechadas por el uso culto." But is not such conservatism

due rather to isolation than to class? The pronunciation of French orthographic *oi* as [wɛ], today dialectal, was first discarded by the Parisian working classes, and retained longest in "uso culto" by the provincial gentry and small nobility; and many alterations —of pronunciation, grammatical form and vocabulary—in the speech of a ruling caste or class can be shown to have a similar history. Certainly, *lig* of Yorkshire dialectal *lig thee daan* 'lie down' is more conservative than the standard English form; but in the formation of the same verb's past tense, *ligged* 'lay', the reverse is true. Moreover, as Álvarez himself says, the majority of archaisms still current in Puerto Rico can be found also in the rural or dialectal speech of other American countries and of Spain itself.

Outstanding phonetic characteristics of Puerto Rican speech, not generally shown in the standard Spanish orthography employed throughout this book, are (18):

> el seseo, el yeísmo, la omisión del sonido de *d* en final de palabra y en posición intervocal (en el habla popular y familiar), la pronunciación de *h* aspirada en lugar de la *j* castellana y de la *s* en final de sílaba, el empleo de *s* dental en lugar de *s* alveolar castellana; la velarización de *n*. Pasamos por alto otros rasgos prosódicos de aparición esporádica y parcial en la Isla, como el uso de *rr* velar en lugar de la correspondiente alveolar, y el intercambio de los sonidos de *l* y *r* en posición final de sílaba.

Subsisting archaisms appear particularly in the pronunciation —in etymological forms like: *creba(d)a* 'quebrada', *cuasi* 'casi', *dicir* 'decir', *enjaguar* 'enjuagar', *mesmo* 'mismo', *agora* 'ahora', *falcón* 'halcón', *lamber* 'lamer', *murciégalo* 'murciélago', *riseras* 'tijeras'; and in assimilating and dissimilating forms like: *añidir* 'añadir', *invidia* 'envidia', *escrebir* 'escribir', *herver* 'hervir', *cencia* 'ciencia', *celebro*, 'cerebro', *peligrino* 'peregrino', *semos* 'somos'. Aspirate *h* has been retained as such in a majority of words that contained it, such as: *haba*, *hablar*, *hacer*, *habmre*, *harina*, *hasta*, *heder*, *hembra*, *hender*, *herver*, *hiel*, *hierro* (rarely *fierro*), *hondo*, *horno*,*h uir*, *humo*, *haogar*, *ahorcar*, etc.; and less frequently in others such as *hijo* and *hoja*, where its loss may be explained by dissimila tion. It also occurs in some cases where the standard language has retained *f;* so: *hué* 'fué', *huí* 'fuí', *huerte* 'fuerte' and *ahuera* 'afíera'. Final *l* (standard *r*) of the infinitive plus *l* of the enclitic pronoun *lo* gave *-ll-* = *-y-*, as in *sabeyo* 'saberlo'; but this has been replaced

within the past fifty years by *-l): sabelo.* Prothesis occurs in *dir* 'ir', *dentrar* 'entrar' and *manque* 'aunque' (said to be a cross of *masque* or of *maguer* with *aunque)*; while epenthesis, syncope and dislocation of stress are common; so: *g(u)rupa, lig(ue)re, culeca* 'clueca' (this last form being normal in "uso culto" and therefore not archaic), *disparecer* 'desaparecer', *por ái* 'por ahí', *cáido* 'caído', *mái* 'maíz', *réi* 'raíz'.

This last noun has the plural *réise;* while nouns having stress on the final syllable tend to form their plurals in *-ses* (*ajises* 'ajíes', *pieses* 'pies', *cafeses* 'cafés'), and *narices* 'nose(s)' has singular as well as plural reference. There is a preference for the feminine gender in such forms as *la* (or *una*) *almibar, calor, color, mar, reuma;* and expressions like *una poca de agua, una poquita de azúcar* are common; while *la* and *una* are of course employed before feminine nouns having initial aspirate, such as *habla, hacha, hambre*, etc. Archaic nominalizing suffixes are employed in such forms as *compararanza* 'comparación', *conocencia* 'conocimiento', *nacencia* and *nación* 'nacimiento'. 'Strong' participles such as *canso* 'cansado', *hincho* 'hinchado' and *pago* 'pagado' are employed with adjectival function, the last even in "uso culto". An interesting feature of the morphology is the substitution of *-nos* for *-mos* of the first person plural in the formation of verbs such as: *tenianos, estábanos, compráranos, háyanos, séanos* and *véanos;* the last three also showing dislocation of stress. Analogical levelling accounts for verb forms like *haberé* 'habré', *haberá* 'habrá', *habería* 'habría' and *habemos* 'hemos'; while to the same tendency may probably be attributed 2nd person sg. preterites in *-stes* (by analogy with 2nd pl. in *-steis*) and the repetition or transposition of the verbal pluralizer *-n* in such forms as *sientensen* and *sientesen* '*sientense*'. The forms *truje, trujo*, etc. survive from the mediaeval and classical conjugation of *traer;* the old preterite *vide* 'vi' is still heard, though less frequently than the reduced form *veí;* and *diz que* 'it is said that' lives on as *i que.* The adverb *donde* may be replaced by *onde*, the preposition *desde* by *dende* (Lat. *dēinde*).

It is interesting to note that many of the above-mentioned features are found also in the Spanish dialect of Trinidad, now rapidly approaching extinction (*v.* Robert Wallace Thompson, A Preliminary Survey of the Spanish Dialect of Trinidad; in *Orbis* VI.353-372). Thus, with the exception of what is said concerning velar *rr*, Álvarez' remarks on the outstanding phonetic characteristics of Puerto Rican speech (see passage cited above) apply equally well to Trinidadian Spanish. Also in Trinidad aspirate

h has been retained, and *j* has fallen together with it as in: *hiho*, *muhel*, *holno*, *hacé(l)*, *harina*, *ahuera*, *huí*, *hué*, *sy a hogao* 'se ha ahogado'. Likewise, *-rl-* of the standard language is reduced to *-l-*, as in *bula* 'burla'; and we find assimilating and dissimilating forms like *ciluela* 'ciruela, *semos* 'somos' and *creensa* 'creencia'. 'Prothetic *d*' occurs in the same forms as in Puerto Rico, *di(l)* 'ir' and *dentrá(l)* 'entrar'. Trinidadian *molciégalo* corresponds to Puerto Rican *murciégalo;* and *bacanza* 'vacación' of the former, in which Thompson suspects French influence, is comparable to *comparanza* of the latter. Also *-nos* for *-mos* occurs in Trinidad (*hanos* 'hemos'); as do: *vide* 'vi', *vido* 'vió', *asina* 'así' and *dende* 'desde'. The nouns *costumbre* and *mar* are treated as feminine; and while *reuma* is not cited, it seems worth mentioning that also French *rhume* must have been brought to these islands as feminine, since it has given French Creole *larim*, with the same meaning.

Álvarez lists and discusses some 155 lexical archaisms of peninsular ancestry, 28 said to be borrowed from native languages and 31 from African languages. Among the former may be mentioned: *aparar* 'to catch (as a ball)', *cimarrón* 'fugitive slave', or, by extension, 'animal or plant that has gone wild—escaped, that is to say, from domestication or cultivation'; *mancar* 'to miss', *mentar* 'to mention', *querencia* 'affection' and *taita* (also in Trinidad) 'father'. The first of these is reminiscent of French Creole *pare* (Fr. *parer*), which also means 'to stop or catch (as a ball)', as well as 'to avoid, to shelter from (the rain)' and 'to prepare or get ready'. The second, from Sp. *cimarra* 'bush, jungle', is the source of borrowed Fr. *marron*, French Creole *mawó* and Eng. *maroon* with the same meanings.

Deserving of special mention are the forms: *pae*, *pai* 'padre', *mae* 'madre', *compae*, *compai* 'comparde', *comae*, *comai* 'comadre', recorded in Santo Domingo as well as in Puerto Rico and which, Álvarez tells us, Henríquez Ureña considers to be 'lusitanismos' brought to the islands in the 16th century by slaves of the Portuguese. Thompson recorded *compae* and *comae* as modes of address employed in Trinidad; and French Creole—in Dominica at any rate—has: *pai*, *kōpai* and *mai* (through not, I think, **kōmai*) as affectionate forms of address, distinct from *pè* 'priest' ('father' is *papa*, *pap* or *pa*), *kōpè* and *makumè* 'one's child's sponsor, or fellow-sponsor, at baptism (*makumè* also means: effeminate man, male homosexual)'.

It is interesting to learn that *burén* 'cassava griddle' and *conuco* 'small farm or peasant holding', both borrowed from Taino, are

still current; and it is to be hoped that Dr. Álvarez will find time to make a complete list of Taino words still heard in Puerto Rican Spanish; for many of those which are not yet archaic there, such as *ají* 'capsicum', *cabuya* 'noose' (also in French Creole), *cocuyo* or *cucuyo* 'elatarid beetle of the genus *Pyrophorus*' and *guayábana* 'sour sop, *Annona muricata*', are probably little known in Spain and in other parts of the Spanish-speaking world. The word *guayuco* 'old or worn-out garment used only for doing housework, or by children when playing' may also have been borrowed from Taino. Similar forms with similar meanings, such as *way(u)ko* 'loin-cloth', etc., occur both in Arawakan and in Cariban languages; and in French Creole of Dominica (and perhaps elsewhere), *wayku* means 'old cloth or cache-sexe of any sort used when bathing in the river, etc.'. Also *muleque* 'Negro slave boy of six to fourteen years old' appears to be of Indian (probably Cariban) ancestry, and not African as stated; cf. Island Carib *mulékï* 'little boy' and Karina *mureko* 'child'. The adjective *malungo, -ga*, said of a 'gallo o gallina grande', appears to be related to Trinidadian Spanish *maluco, -ca*, with the same meaning, which Thompson took to be a variant of Sp. *malucho;* but both may well be African. Also probably of African ancestry is *calahú* 'dish of minced and boiled herbs'; with which compare W. I. French and English *calalou*, Fr. Creole *kalalu*, which is the name for a kind of herb and of a thick soup made from it and other ingredients.

Dominica, British West Indies. DOUGLAS TAYLOR.

Livro de Soliloquio de Sancto Agostinho (Cód. Alcob. CCLXXIII-198). Edição crítica e glossário por Maria Adelaide Valle CINTRA. (Publicações do Centro de Estudos Filológicos, 6). XXIV-186 p. Lisboa, 1957.

Ludolfo Cartusiano, *O Livro de Vita Christi* em lingoagem português. Edição fac-similar e crítica do incunábulo de 1495 cotejado com os apógrafos por Augusto MAGNE, S. J. Volume I. Rio de Janeiro, Casa de Rui Barbosa, 1957, XXII+506 p., nbr. fac-similés (Coleção de textos da língua portuguêsa arcaica, 1).

Ces deux publications répondent, au Portugal et au Brésil, à une même préoccupation : fournir aux philologues des éditions critiques de textes portugais médiévaux.

La traduction des "soliloquiae animae ad Deum" (apocryphe du XIII^e s.) est conservée dans un manuscrit du XV^e s., incomplet. L'œuvre est déjà citée dans le *Livro da Montaria*, compilé entre 1415 et 1433, et l'éditeur estime que les caractéristiques linguistiques de la traduction la font bien situer dans le premier quart du XV^e s. : un exemple de *-ão* <-ANT, plusieurs confusions de *-om* et *-am* <-ONE ou -UNT, et un cas de chute du *-d-* dans *perguntees*, phénomène attesté seulement en 1410. Le texte, très soigneusement édité, est accompagné du modèle latin lorsque le manuscrit offre des lacunes. Le glossaire, qui occupe 94 pages, renferme toutes les formes du texte, avec référence aux passages, et mention du mot latin qui lui correspond dans l'original. Ce long travail aurait peut-être pu être exploité en outre pour présenter un tableau de certains faits grammaticaux. Par exemple, les formes *aquel(l)* et *aquelle* sont-elles conditionnées par tel emploi syntactique ? Il ne le semble pas d'après *aquel que quer sentir* et *aquelle que nõ deseja;* de même dans le cas de *el-elle*, pour lequel on trouve *cõ el*, *ẽ el* et *delle*, *per elle*. On remarquera que les formes ont été fortement influencées par les modèles latins : INTERLUCERE = *entreluzir*, EXAUDIRE = *exouvir*, mais par contre OCCULTE = *encuberlamente* et DELECTATIO = *dulçura*, alors que *deleytaçõoes* est utilisé dans un autre passage. Le traducteur a tenu également à doter les adjectifs en -US de la terminaison portugaise *-al:* DIUINUS = *divinal*, ANGELICUS = *angelical*, CELESTIS = *celestial*, MUNDANUS = *mundanal*. Grâce à ce lexique complet, on pourra étudier d'autre part des couples tels que *luz-lume*, ou *cuidar-pensar*, etc., (p. 93, s. v. *afeytar*, « apreyta », lire « spreyta ».

Ludolfo a écrit sa *Vita Christi* vers le milieu du XIV^e s. ; vite célèbre, elle a été éditée une centaine de fois, et traduite en plusieurs langues. La traduction portugaise a été terminée en 1466, et imprimée en 1495 à Lisbonne. Le P. A. Magne donne sur les pages paires le fac-similé de l'incunable, et sur les pages impaires le texte du manuscrit, incomplet en certains endroits. On note entre les deux de nombreuses alternances graphiques et des variantes formelles. Cette publication doit comprendre six volumes de texte, le septième étant constitué par le Glossaire exhaustif de cette traduction. Sagement, le P. A. Magne consacre déjà 70 pages au commentaire de formes anciennes. Ce glossaire sélectif est instructif par lui-même ; les citations transcrites permettent de préciser de nombreux sens, comme ceux de *luz* 'lumière pure' et *lume* 'source de clarté mêlée à une autre chose', nettement distingués dans la *Vita Christi*, et qui rendent comptent des emplois de ces mots dans le *Soliloquio* entre autres.

Il faut au linguiste une documentation précise et sûre : ces types de publications lui sont d'un grand secours.

Université de Strasbourg. BERNARD POTTIER.

PIERRE FOUCHÉ, *Traité de prononciation française.* LXIII+528 pp. Paris: Klincksieck, 1956.

Like most of the existing works on the pronunciation of contemporary French, this book has an essentially normative orientation. It seems clear that the author is more concerned with the setting up of a model based on the "careful conversational speech of cultivated Parisians" rather than with the description of this kind of speech.

The main discernable differences between this work and its predecessors are 1) its size (almost 600 pages), and 2) the great number of foreign words and proper names, both French and foreign, cited.

The first part of the book, entitled *Introduction* (IX-LXIII), deals with the sounds of this received French. For each sound listed (by means of IPA symbols), examples of the closest phonetic equivalents in the main European languages are given. The sounds noted as distinct by Prof. Fouché coincide rather closely with the units a phonemic analysis would yield. This, of course, is not surprising since the author is a member of the French linguistic community. However, he makes no use, at least consciously, of the notion of distinctive function. The closest he comes is when he states (page 84) that one should not confuse *mâtin* [mɑtɛ̃] « gros chien » and *matin* [matɛ̃] «partie de la journée ».

The distinction between the phonemic and the non-phonemic can thus not be clearly made. Non-phonemic length is usually noted very carefully, e.g. *chaude* [ʃo:d], *décor* [dekɔ:r], *atome* [ato:m]. It is therefore surprising to find no mention of a frequently heard and orthoepically recommendable phonemic distinction based on length between ɛ̆ and ɛ̃, as in the words *mettre* and *maître* respectively.

On the other hand, the inclusion of œ̃ as a distinct sound does not seem entirely justified from a descriptive viewpoint. A very large number of Parisians, both cultivated and uncultivated, do not use this sound. They pronounce the numeral *un* [ɛ̃], and *brin* and *brun* exactly alike [brɛ̃].

The vowel system given is undoubtedly used by a certain number of cultivated Parisian speakers, but it seems difficult at the present time to justify its choice as the "best" one or the preponderant one. Actually, little is known, scientifically speaking, about the pronunciation of Parisian French. It is a domain that has never been subjected to a serious detailed descriptive treatment with full use made of phonological and statistical procedures and with orthoepic considerations left in the background. (Martinet's book, *La prononciation du français contemporain*, Paris, 1946, is along these principles but it is not limited to Parisian speech.) The book under review, based, as the author tells us, on a certain sampling, could have made a real contribution to our knowledge of cultivated Parisian speech, which for centuries has been the standard for "correct" French pronunciation. Unfortunately, the procedures and methods adopted are those of the prephonological era. This might have been justified for pedagogical reasons; one generally does not innovate in reference books. However, here, the weakness of the general principles underlying the exposition render difficult a clear, precise presentation of the facts.

The treatment of geminate consonants can be taken as an example. A foreigner wishing to improve his pronunciation of French would probably be led to believe that French, like Italian, is a language in which gemination plays an important rôle, and that it is necessary for him to learn to distinguish in his pronunciation between geminate and single consonants. To learn the long lists of words, all written with double letters but some of which are noted as being pronounced with a double consonant and others with a single consonant (e.g . *allaiter*, *arriver* with geminates, but *addition*, *attacher* with single consonants) would require a considerable feat of memorization. Fortunately for the student, this effort is unnecessary in actual fact. Gemination does exist in French, but, generally speaking, it does not have a distinctive value in relation to the single consonant. It is a variant used in words spelled with a double consonant (double letter); or else, it results from the coming together at a morpheme boundary of two like consonants (for example, *nettelé* [nɛt-te]). It is in this latter case that gemination seems to have a more stable existence, yet the author has not noted examples like *mourrez* or *courrez* with [R·] or [RR] as distinct from *mourez* and *courez* with [R]. If, instead of imposing the geminate pronunciations listed, the author had somewhere mentioned the optional nature of almost all geminates in French he would have been closer to the linguistic reality. Non-

gemination is quite acceptable linguistically and socially. The foreign student, particularly one whose native language has no geminates, would have been extremely grateful for such a statement which would ease his task of learning without a sacrifice in quality.

The introduction is the only part of the book that deals directly with phonetics and pronunciation as such. The main body of the work is devoted mostly to the correspondence rules between French spelling and pronunciation. It is, of course, convenient to be able to look up the pronunciation of a word whose spelling alone is known. Here, however, the material is classed half phonetically half alphabetically, letter by letter. To find the pronunciation of a borrowed word like *boarding-house*, it is necessary to look successively under the spellings *oa*, *ing*, *h* and *ou*. In addition, the index of terms cited is far from exhaustive. In view of the author's expressed desire to be as practical as possible it is hard to understand why he chose a plan which makes consultation so complicated. Since the principal aim of the book is to tell how one ought to pronounce a given word, a pronouncing dictionary of the type written for English by Daniel Jones would have offered the eventual user a much more convenient and manageable reference work.

One of the main innovations of the book under review is the considerable space devoted to foreign words and names. It is very sensibly proposed that these be accented, as is usual in French, on the last syllable. However, it is more difficult to follow the author when he asks Frenchmen to pronounce phonemes quite foreign to French such as [x] or [hg] (?) in Spanish words (e.g. *Juarez*, *Jota*, *jaléo*) as [θ] in English and Spanish words *(Mathew*, *Southampton; zapatero)*. This tendency towards conformity with the original language is most pronounced for Spanish and Catalan, languages taught by the author, and, to a slightly lesser extent, for English.

Most of the foreign words and names listed are marginal to the French language. They are foreign words, not borrowings, and as such, they can hardly be considered as integrated in the lexicon proper of the language. In addition, they are usually read, rarely pronounced. To try to set up standards for their pronunciation is a very difficult task from a descriptive viewpoint. In spite of greatly increased bilingualism (English is becoming widely known) particularly among the younger generations since the end of the war, it seems impractical, at the present time at

least, to wish to impose pronunciations containing phonemes foreign to French. When pronounced imperfectly by the speaker, as is most often the case, they produce an effect that is either amusing or pretentious. Personal experience has shown that the foreigner generally does not understand the word or name supposedly pronounced according to the canons of his language, and that the Frenchman, particularly one who is unacquainted with the foreign language in question, does not understand either. The clearest type of signal by far, at least for these "eye" words is one which allows the hearer and the speaker to correlate as much as possible the spelling and the pronunciation. This would mean, generally, pronouncing the written word *à la française*, that is, interpreting the spelling in accordance with the rules and habits of French. This is what the author normally suggests for "exotic" (Chinese, Vietnamese, etc.) words and names. This is the way native French speakers usually treat words from foreign languages known through written media. This principle, of course, would have to be applied in a flexible manner, and admit exceptions.

From a more general viewpoint, what seems most striking about this book is its anachronistic character. Thirty years have now passed since 1928, the year in which the First Congress of Linguists was held. This date can be considered for practical purposes as marking the birth of phonology. Since then literally thousands of articles and books have served to illustrate concretely the great progress that has been made in the study of the sounds of language through the application of phonological (phonemic) methods. The conscious application of these methods has tended to free the study of language sounds from subjective interpretation on the part of the linguist and has introduced objective criteria for determining and classifying systematically and hierarchically phonic differences of various functional types existing in a given language. In this book, the author has chosen to disregard just about all that has been done in phonology through the years. It would be very unfair to criticize him because he is not a practicing structural linguist. However, a little bit of eclecticism on his part instead of a resolutely ostrich-like attitude towards everything phonological probably would have resulted in a much more satisfying and valuable book by introducing more clarity, exactitude and concision.

Paris, C. N. R. S. STANLEY LAMPACH.

MAX LEOPOLD WAGNER, *Dizionario etimologico sardo*, dispensa 2 *(ammeditlsare-babbalúkka)*. Sammlung romanischer Elementar- und Handbücher. Heidelberg: Carl Winter, 1958.

En rendant compte du premier fascicule de cet ouvrage, j'avais signalé les qualités remarquables de ce dictionnaire qui fait honneur à son illustre auteur. Comme l'on pouvait s'y attendre, on retrouve dans le nouveau fascicule la même richesse d'information, la même clarté, la même heureuse mesure de jugement.

Je me permettrai d'attirer l'attention sur un seul point qui, à mon avis, met bien en lumière les difficultés auxquelles se heurte l'étymologie dès que le comparatiste ne travaille plus avec des langues dont les rapports historiques ou préhistoriques sont bien établis. Dans ce terrain dangereux, où il faut bien s'aventurer quand même, ce qui semble clair au premier abord se brouille très souvent, si l'on regarde les faits de plus près.

Il s'agit du sarde (Barbagia) *aùrri* "carpino nero", qui serait apparenté au bsq. *aurri* « plante qui a les racines longues et les feuilles larges ».

« Il parallelismo è certamente singolare », écrit M. Wagner, ce que personne ne niera, pourvu qu'on se tienne à la forme. Car le mot basque est isolé. On n'y peut rattacher, comme le voulait Bertoldi, le labourdin (ou bas-navarrais) de Bardos *aurrede* « oseraie sauvage » (Azkue), dont la finale a été changée en *-di* par le P. Lhande, simplement parce que *-di* forme des collectifs, tout comme lat. *-ētum*. Du reste, cette analyse est évidemment incompatible avec l'opinion émise par Lhande à la même place, d'après laquelle *aurrede* ne serait qu'un emprunt au gasc. *aubredé*.

Or, cette dernière opinion a toute chance d'être correcte : il n'y a rien d'étonnant à ce que le gascon de Bayonne *aubredé* « plant d'aubiers, saussaie » (S. Palay), dont l'introduction en basque doit être récente, y ait été modifié en *aurrede*. Il est bon de noter aussi que le caractère roman d'*aubredé* ne fait aucun doute : cf. gasc. *aubadé*, *aubadére*, *aubaréde* « plantation d'aubiers, de saules, etc. », v. gasc. *aubareda*, v. prov. *albareda* « lieu planté de saules blancs » (*FEW* I, p. 59 s., *REW* 317 et 318). Le même collectif roman est à la base de bsq. *obrada(lze)* 'espèce de saule' (K. Bouda et D. Baumgartl, *Nombres vascos de las plantas*, Salamanque 1955, p. 14). Il faudrait rapprocher encore de ce groupe le b.-nav. et souletin *auga(dera)* « osier sauvage », soul. *augatze* « populus tremula » (gasc. *aubà* « arbre à bois blanc tel que le saule »). La permutation $b \sim g$ est très fréquente en basque avant ou après voyelle labiale : cf. bsq. *augeta* 'aubade' (*FEW* I, p. 63 a).

Bsq. *aurri* est donc isolé. Il n'a pas d'histoire : les ouvrages spéciaux consacrés aux noms des plantes (Althabe, Lacoizqueta) n'en font pas mention. Azkue lui-même, qui a entendu ce mot dans une seule localité du domaine haut-navarrais, a été incapable de donner une description précise de la plante qu'il désigne. Qui plus est, s'il n'est pas niable que cette localité existe, puisque Azkue l'a citée à plusieurs reprises, je n'arrive à la retrouver nulle part. Il est vrai que *aurri* est attesté dès le XVII^e siècle, mais avec des valeurs trop divergentes (« maison qui n'a que le toit et les murailles », selon Oihenart) ; *-aurri* reparaît à la finale de quelques noms de plantes (hièble, bruyère, Phyllirea media), mais l'analyse en est incertaine. Il serait donc trop hasardeux de rapprocher de quoi que ce soit un mot dont le sens, la localisation, voire la réalité sont douteux. On devra différer le jugement jusqu'à ce que les faits basques soient mieux établis.

Quelques observations de détail. Il n'est pas sûr, malgré Schuchardt, que l'aspiration du basque-français *ahate* « canard » suppose une accentuation latine **anáte*, tout comme sard. *anáte*, etc. Il y a en basque pas mal de mots d'emprunt anciens, tels que *bake* « paix », *zeta* « soie », etc., où l'aspiration ne s'est pas produite après l'accent latin, mais il y en a aussi d'autres où, malgré cela, l'aspiration est présente : *bikhe* « poix », *liho* « lin », etc. Il ne faut pas oublier que, dans les mots qui ne sont pas des emprunts au latin, *h* répond de règle (en souletin, par exemple) à un plus ancien *n* à l'intervocalique, quand cette consonne était placée, comme dans ce cas, entre la première et la seconde syllabes du mot (cf. A. Martinet, *Économie des changements phonétiques*, p. 383 s.) : *ihizi* « chasse, gibier », etc. Si l'on doit y voir un effet de la position de l'accent, il suffirait de supposer que certains emprunts ont été accentués d'après le type normal en basque ancien, tandis que d'autres ont conservé l'accent à sa place originelle.

Le camp. *armas de Déus* « i sacramenti che si amministrano agli infermi di malattia grave » s'expliquerait parce qu'on aurait considéré les sacrements comme des armes contre la maladie. Ne seraient-ils pas plutôt, selon la conception catholique, des armes surnaturelles qui permettront au malade de livrer avec succès le dernier combat contre ses ennemis spirituels?

asta « corne » est aussi et surtout castillan (cf. Corominas s.v.), ce qui pourrait être en rapport avec les faits sardes.

P. 111 b, l. 2, lire esp. *argüir*. P. 151 b, l. 8, lire bsq. *andere*.

Luis Michelena.

Academia Republicii Populare Romîne, Institutul de Lingvistică din Bucureşti, *Dicţionar ortoepic*, 1956.

Ce dictionnaire orthoépique se propose d'éliminer les prononciations dialectales et de propager la prononciation qu'emploient dans le style soutenu les personnes cultivées de Bucarest. Une introduction est consacrée à souligner les fautes les plus courantes : étendre aux néologismes la prononciation *je ji wo* de *e i o* en position initiale de syllabe ; diphtonguer *e* et *o* accentués après une consonne; centraliser *e* et *i* en *ă* et *î* après *ţ s z ş j;* diphtonguer certaines voyelles *(a>ea)* ou modifier leur timbre *(î>i)* après *ş* et *j;* fermer *a* et *e* atones en *î* et *i;* monophtonguer certaines diphtongues: *ea>a, ie>e, oa>o;* ne pas distinguer nettement *ia* et *ea*, etc... En ce qui concerne les consonnes, le dictionnaire recommande de ne pas confondre sourdes et sonores en finale, de distinguer nettement les consonnes mouillées des consonnes dures et d'éviter la palatalisation des consonnes devant *e ea, i ie*. On recommande enfin d'éviter l'omission, propre au style allegro, de certaines voyelles internes : *i e ă*, et de certaines consonnes finales, en particulier *-l* de l'article. La seconde partie de l'introduction est consacrée aux erreurs morphologiques : de nombreux mots ont deux pluriels — le dictionnaire indiquera lequel s'impose par sa fréquence ; on remet en mémoire la déclinaison de certains pronoms et leur emploi ; on étudie également certains problèmes posés par les verbes.

La liste de mots qui forme le corps du livre ne donne pas de transcriptions phonétiques (ou phonologiques) mais indique seulement, à côté des pluriels délicats et des formes verbales irrégulières, l'accentuation et la syllabation.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

AL. ROSETTI, *Limba romînă în secolele al XIII-lea al XVI-lea;* Materiale şi cercetari lingvistice, Editura Academiei Republicii Populare Romîne, 1956.

Al. Rosetti indique dans la préface que son livre contient des matériaux publiés dans le sixième volume de son *Histoire de la langue roumaine* (1946) ainsi que des informations extraites des publications postérieures à 1946.

Une introduction est consacrée à la méthode employée par

l'auteur pour étudier les graphies. Il s'inspire des principes de la phonologie et tient compte (p. 23) de l'orthographe employée à l'époque en question et aux époques précédentes ; de la fréquence des graphies bizarres dans des textes différents et chez le même auteur ; du degré de culture du scribe ; du fait qu'en un intervalle de temps court, la langue ne peut pas changer beaucoup et du fait qu'il y a des graphies qui échappent à toute explication.

Le chapitre consacré à la langue du XIII^e siècle est forcément très court, faute de documents : les témoignages sont extraits de textes en latin ou en slave ancien. En ce qui concerne le consonantisme, on remarque cependant que les dialectes de Valachie et du sud de la Transylvanie sont plus avancés que ceux de Moldavie.

A la langue du XVI^e siècle, Rosetti consacre une étude trés détaillée ayant trait successivement à la phonétique, la morphologie, la syntaxe, la stylistique et le vocabulaire. Les faits sont présentés groupés par région d'origine des documents. En réalité, il est difficile d'attribuer un phonétisme donné à une région déterminée, mais il y a trois blocs : les deux premiers (1· Moldavie, Transylvanie du N., Maramureş, 2. Valachie, Trans. du S.) s'opposent constamment, tandis que le Banat se range tantôt avec le premier, tantôt avec le second.

Les textes permettent de suivre le progrès de certaines innovations phonétiques au cours des XVI^e et XVII^e siècles : palatalisation des occlusives labiales et des fricatives labio-dentales, passage de *ñ* à yod. On remarque également le caractère archaïque de la morphologie, qui fait ressortir l'ampleur du travail de nivellement opéré par l'analogie dans les dernières phases de l'histoire du roumain.

L'ouvrage se termine par deux appendices dont le premier est consacré aux problèmes de datation et localisation des plus anciennes traductions roumaines de textes religieux, passant en revue les théories proposées entre 1915 et 1943. Rosetti montre que les différences entre ces traductions et la langue de Coresi ne s'expliquent pas par le décalage dans le temps, mais par la distribution dans l'espace et que par conséquent les plus anciennes traductions ne sauraient remonter au-delà du XVI^e siècle. En faveur de la localisation dans le Maramureş et le nord de la Transylvanie, Rosetti apporte des arguments d'ordre linguistique, consistant en mots magyars employés dans les textes rotacisants et existant dans les dialectes actuels de ces régions, et des arguments d'ordre historique : les livres saints ont été traduits dans le nord de la Transylvanie au début du XVI^e siècle sous l'influence de la propa-

gande luthérienne, et non pas en Valachie où l'église de langue slave veillait à l'orthodoxie, cf. intr. à la seconde partie, p. 51-61.

Le deuxième appendice montre, par la juxtaposition de passages identiques, que, pour rédiger son *Apôtre*, Coresi a eu dans les mains un texte semblable au Codice Voroneţean et qu'il a collationné cette version roumaine avec la version slave.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

Academia Republicii Populare Romîne, Institutul de Lingvistică, *Contribuţii la Istoria limbii romîne literare în secolul al XIX-lea*. Editura Academiei Republicii Populare Romîne, 1956.

En réponse aux nombreuses recherches faites sur les phases anciennes de la langue roumaine, le besoin s'est récemment fait sentir d'études consacrées à la langue littéraire du XIX^e^ siècle. La collection d'articles publiée par l'Académie R. P. R. répond à ce besoin. Elle forme un ensemble avec les deux volumes de la Grammaire de la même Académie et avec son dictionnaire. Ces contributions serviront ultérieurement à l'élaboration d'une histoire de la langue roumaine au XIX^e^ siècle.

La collection comprend neuf articles dont huit étudient la langue, ou certains aspects de la langue, d'un auteur, et dont l'un (Gr. Ivănescu) est consacré à la formation de la terminologie philosophique moderne. La plupart des essais traitent surtout de vocabulaire et de grammaire, le dernier (« Structure de la phrase dans le style de B. Delavrancea », Al. Niculescu) est consacré entièrement à la stylistique, tandis que Gh. Bulgăr (« Particularités de langue et de style dans l'œuvre de C. Bolliac ») et Tudor Vianu (« Observations sur la langue et le style de A. I. Odobescu ») s'occupent à la fois de vocabulaire, de grammaire et de stylistique.

Dans le premier essai (« Langue des poèmes de B. P. Mumuleanu ») Gr. Brîncuş montre que la base de la langue du poète est la langue parlée dont il s'efforce de reproduire les particularités phonétiques, dont il utilise le vocabulaire et les tournures syntaxiques : par ex., pour exprimer le complément indirect, *la* remplace le datif. Il souligne également les archaïsmes (*nici* employé sans *nu*, *de leac* fréquemment employé comme négation, etc.), les quelques traits dialectaux méridionaux et les emprunts ou calques étrangers (*alîl* au lieu de *altîl de*, sur le modèle français).

Dans leur article sur les « Néologismes dans les œuvres de

Anton Pann », B. Cazacu et I. Fischer remarquent que le changement d'orientation sociale correspondant à la libération de la Moldavie et de la Munténie et à la décadence du féodalisme appelle la création d'un vocabulaire nouveau ; ils dressent une liste des mots empruntés : ceux-ci appartiennent aux domaines de la typographie et de l'édition, de l'enseignement, des sciences, de l'art (musique et théâtre empruntés surtout à l'italien). Le russe fournit directement ou indirectement la majorité des termes du domaine économique, administratif, politique et militaire, et le français, des mots ayant trait à la vie mondaine.

Al. Rosetti (« La langue des œuvres de Ion Heliade Rădulescu « jusqu'en 1841 ») insiste sur la préoccupation de Rădulescu de constituer une langue littéraire générale à partir de la langue des livres religieux. Kogălniceanu s'inquiète surtout de l'unité de la langue littéraire et la multiplicité de ses activités lui permet de jouer dans ce sens un rôle important, cf. Dan Simonescu « La contribution de M. Kogălniceanu au développement et à l'enrichissement de la langue littéraire ».

Iorgu Iordan choisit d'étudier l'aspect populaire de la langue de Creangă. Cette couleur populaire résulte de divers procédés : emploi du dialogue fictif avec le lecteur, emploi de l'exclamation et de certaines interjections ; répétitions, reprise des questions par l'interlocuteur, nature des comparaisons, emploi d'expressions idiomatiques, de proverbes, de diminutifs à sens augmentatif. Comme traits populaires strictement linguistiques, il faut mentionner la prédominance de la coordination sur la subordination, l'emploi très fréquent du présent « dramatique » et le choix du vocabulaire.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

Academia Republicii Populare Romîne, Institutul de Lingvistică *din Bucureşti, Limba Romînă, Fonetica—Vocabular—Gramatica ;* Editura Academiei Republicii Populare Romîne, 1956.

A la différence de la *Gramatica limbii romîne* de la même Académie (1954) ce traité de langue roumaine ne s'adresse pas aux spécialistes, professeurs de langue roumaine et philologues, mais aux étudiants de l'enseignement secondaire et en général à tout homme désireux de se cultiver ; en effet il n'est pas consacré exclusivement à la grammaire, mais à la langue roumaine dans son

ensemble, et contient par exemple des notions d'histoire du roumain. Les auteurs, partant de la Grammaire, ont amplifié la section sur le vocabulaire, simplifié les sections de morphologie et de syntaxe et réduit considérablement la partie phonétique, mêlant à la description des intentions normatives.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

AURELIO RAUTA, *Gramatica Rumana*, prólogo de Cesar Real de la Riva ; Tesis y estudios salmantinos IV ; Consejo superior de investigaciones científicas, Colegio trilingüe ; Universidad de Salamanca. 1947.

Cette grammaire roumaine a pour premier mérite d'être la première qui soit publiée à l'usage des hispanophones. Le corps de la Grammaire est encadré d'une part par une préface de Don César Real de la Riva qui situe la communauté de langue roumaine dans l'histoire et donne un aperçu des caractéristiques du roumain par rapport aux autres langues romanes, et d'autre part par un appendice de l'auteur qui présente des données sur la dispersion des peuples de langue roumaine dans le monde et sur les groupes allogènes qui se sont implantés — en conservant leur langue — sur le territoire roumain.

Le genre de l'ouvrage est intermédiaire entre la grammaire et le manuel. Ce n'est pas un véritable traité de grammaire en ce sens que la phonétique et la morphologie ne sont pas présentées de façon systématique et objective. Ce n'est pas non plus un véritable manuel, car il n'y a pas de principe pédagogique évident : les exercices qui sont proposés dans chaque chapitre ne s'appliquent pas suffisamment à leur contenu ; il y a trop de listes indigestes avec pour seule méthode de classification le principe alphabétique, cf. la liste des conjonctions (ch. XIV), celle des prépositions (ch. XII) où, au lieu d'essayer de définir et de classer les emplois, Rauta se contente de traductions approximatives accompagnées de listes d'exemples.

Dans le chapitre sur la phonétique, A. Rauta renonce à une définition du son *î*, renvoyant les étudiants à l'imitation du professeur. Or, il me semble que même si ce son est difficile à reproduire pour un hispanophone ou un francophone, il n'en reste pas moins descriptible en termes de degré d'ouverture et de point d'articulation. Sans doute pour éviter d'effrayer le débutant de termes

techniques (souci qui l'abandonne cependant en ce qui concerne la syntaxe) il néglige la distinction importante pour le roumain entre diphtongues ascendantes *ea eo ia ie io iu oa uă ua* et descendantes *ai au ăi au îi îu ei eu ii iu oi ou ui uu*. De plus si on compare sa liste avec celle de la Grammaire de l'Académie R.P.R., on constate que Rauta a omis au moins quatre diphtongues : asc. *ua*, desc. *îu*, *iu*, *uu*.

En ce qui concerne la morphologie, il y aurait eu avantage à présenter dès le chapitre II (pluriel des noms) la question des alternances vocaliques et consonantiques, au lieu d'attendre le ch. VII (déclinaison). Il me semble également erroné, tant du point de vue pédagogique que du point de vue scientifique de présenter dans une liste, sans commentaire ni analyse, les pluriels des substantifs (féminins et neutres) qui échappent aux deux ou trois règles les plus générales. On a toujours meilleur compte à exprimer les choses en clair et à faire ressortir par exemple que les féminins terminés en *-a* accentué font le pluriel en *-ale*, les fém. en *-ea* accentué, en *-ele*, etc.

Il faut louer la présentation bilingue du début du livre et le passage graduel au seul roumain ; il faut louer également l'adjonction d'un chapitre de syntaxe, bien qu'on se demande pourquoi on a renoncé à le rédiger en roumain.

La grammaire est suivie d'un ample choix de textes : poèmes et prose modernes, poésie populaire ; deux textes anciens ; textes dialectaux : macédo-roumain, mégléno-roumain, istro-roumain, avec traduction en daco-roumain.

L'ouvrage se termine par un lexique précieux par l'abondance du vocabulaire qu'il contient et par l'indication de tous les pluriels. On déplore cependant qu'un lexique à l'usage d'étrangers ne comporte pas d'indications d'accent.

Malgré ces quelques défauts auxquels peut remédier facilement un professeur entendu, la grammaire de Rauta est un bon instrument de travail. Elle contribue cependant à me convaincre que la présentation la plus simple et la plus accessible de la structure d'une langue est toujours la plus scientifique.

Paris, C. N. R. S. GENEVIÈVE CORRÉARD.

STANISLAW WESTFAL: *A Study in Polish Morphology: the Genitive Singular Masculine.* (= Slavic Printings and Reprintings 8.) xx+399 pp. The Hague: Mouton and Co., 1956.

Among the basic problems in "dynamic synchrony" is the unraveling of intersecting sets of criteria for the selection of partic-

ular allomorphs of a given morpheme. The concord class in German called "gender" is a familiar example. In a noun like *Fräulein* 'miss, young lady', the sex of the designatum dictates that gender which semantically connotes femaleness, i.e. the "feminine," hence the variant of the definite article *die*, the pronoun *sie* 'she', the possessive adjective *ihr* 'her' etc. The morphological construction of the noun base, on the other hand, in a conflicting way requires that gender with which the derivational (diminutive) suffix *-lein* is associated, viz. the neuter, and hence the variant of the article *das*, the pronoun *es*, the possessive adjective *sein*, etc. Likewise a noun such as *Sand* 'sand' may be interpreted, from the point of view of its phonological makeup, as analogous with *Band*, *Rand*, *Strand*, which are masculine, or in a conflicting way, with *Hand*, *Wand*, which are feminine. When the criteria for the selection of an allomorph (e.g. *der* vs. *die* vs. *das*) are in conflict—phonological makeup, morphological makeup, and meaning of the noun all pushing in different directions—the descriptive problem is (1) to formulate the criteria with precision, (2) to state their hierarchy, i.e. the order in which they are applied. An alternative approach would be to say that in a language like German, gender—i.e. the selection of allomorphs of the article and of certain other concordant classes—is arbitrary and can only be stated separately for each noun in the dictionary. But this less ambitious procedure would not account for gender assignment—i.e. systematic allomorph selection—as a process which is at least partly productive. The productivity of the process is manifested in actual usage by the deeply rooted fluctuation in gender of certain nouns which apparently lie at the intersections of criteria whose hierarchical interrelation is unspecified. Thus, in normative German grammar, morphological construction of the noun (*-lein* → neuter) takes precedence over meaning ('female' → feminine), but in actual usage this precedence is compromised, leading to fluctuations in the use of *sie* and *es* to refer to *Fräulein*. The productivity of gender assignment could also be tested by introducing neologisms in an experimental situation, where a great number of factors can be controlled. Probably a number of nouns remains whose assignment to gender is completely arbitrary, i.e. purely lexical; but the descriptive task is not completed until the precise structure of the productive, i.e. grammatical, process and its delimitation from the lexical residue, has been stated.

One such problem in allomorph selection by intersecting criteria

is the genitive singular suffix of masculine nouns in Polish, which may be either *-a* or *-u*, depending on—what? To this, "the most difficult problem in the structure of modern Polish" (xix), the present study by Westfal is devoted. The chief merit of the book is the wealth of material it presents—8,652 different nouns, culled from 175,000 pages of Polish text of the years 1900-1950. The autor also makes two important analytic contributions. (1) He demonstrates that the extent of fluctuation between the *-a* and *-u* allomorphs is far greater than even non-normative grammars of Polish would lead one to suppose. (2) He suggests an important new criterion, semantic in nature, for the selection of the allomorphs (see 2c below). But as an attempt at solving the descriptive problem in earnest, the study must be adjudged a failure. Perhaps another worker who will not be daunted by the chaotic organization and undisciplined argumentation of Westfal's book will be able in the future to perform upon his rich materials an analysis that will satisfy descriptive-linguistic standards.

The following major criteria are used by Westfal in accounting for the choice of the *-a* allomorph as against *-u*:

1. Semantic
   a. Animates.
   b. Abstracts.
   c. Tridimensional inanimate objects of the size of man or smaller. (This criterion is Westfal's own discovery.)
2. Morphological
   Nouns ending in certain suffixes, e.g. *-ek*, *-'anin*, *-ciel*, *-us*, etc.
3. Phonological
   Polysyllabic nouns in which the *e* of the second syllable is dropped in the declension, e.g. *sweter—swetr-* 'sweater'. Among the phonological criteria for *-u* are noun base ending in *r* + consonant, base beginning [!] in *e-*, base containing [!] *-ea-*, etc.).

Two minor criteria are listed:

4. Depreciative connotations.
5. Rhyming needs.

In part I, "Detailed Presentation of the Problem" (1-334), Westfal gives his vocabulary of nouns and the information about their genitive forms. The nouns are arranged into about 115 semantic categories, such as chess pieces, parts of the body, "medals, rings, etc.," honeycombs, cemeteries, "(generally indi-

cated) places," metrical feet, persons, animals, "beings," and the like. There is no word index, and a student of Polish interested in information on the genitive of a particular noun would have to search for it with much trial and error. The 115 or so semantic categories are gathered into groups and classes as follows. The list of categories is first dichotomized into animates and inanimates. (The animates do not constitue much of a problem.) The inanimates comprise five classes:

I. Nouns signifying tridimensional objects of the size of man or smaller; these select the genitive allomorph *-a*.

II. Nouns which satisfy the semantic criteria of I but incompletely; these fluctuate between the allomorphs *-a* and *-u* (e.g. carpets, which may be conceived as bidimensional).

III. Concrete nouns *not* signifying tridimensional objects of the size of man or smaller (bidimensional objects, very large tridimensional objects, non-objects such as materials, etc.); these select *-u*.

IV. Abstract nouns others than designations of days; these select *-a*.

V. Designations of days; these select *-u*.

The reader would be mistaken, however, if he expected only *-a* genitives in Class I and IV, only *-u*'s in III and V, etc. Quite the contrary: both *-a*, *-u*, and *-a/-u* fluctuations are found in any semantic category, whatever Class it may belong to. The helplessness of Westfal's plan is best seen by considering in detail the content of one of his semantic categories picked at random. As an example, we take "31. Apparatus, Contrivances, Machines, Tools, etc., and Their Parts" (64-77).

The distribution of genitive allomorphs by nouns in Category 31 is as follows:

Genitive Allomorph	Non-Diminutives	Diminutives	Total
-a	225	52	277
-u	123	2	125
-a or *-u*	55	1	56
Total	403	55	458

This whole category of 458 nouns is assigned to Class I, in which we expect the *-a* allomorph on semantic grounds, yet as much as 61 % of the category does not take exclusively *-a*. Clearly, then, there are criteria of higher *(-u)* or equal (fluctuation)

priority over the class meaning 'tridimensional object ⩽ man'. Since no. 31 is not a "miscellaneous" residue category, but quite typical, it is apparent how unwise it was for Westfal to base the primary organization of his material on the semantic criterion. That meaning operates as a fairly low-order criterion, at best, becomes even more apparent when we find that not only the use of *-u* and *-a*/*-u*, but even the use of *-a* WITHIN Category 31 is accounted for on *non-semantic* grounds in the great majority (88 %) of nouns, as follows (we take only the non-diminutives):

Criterion	Allomorph *-a*	Allomorph *-u*
Morphology (derivational history) of the stem	149	25
Phonology of the stem	39	90
Foreign (Russian) origin of the noun	1	
Semantic extension of nouns of other Classes	9	7
Semantic Class I, as defined	**27**	
"Reason Unclear"		1
TOTAL	225	123

Westfal's utilization of the phonology of the stem as a criterion is naive in the extreme. Two nouns are said to require *-a* because they end in *-un* (67), yet we find six nouns taking the allomorph *-u* because, ending in *-n*, they are "dental stems" (74). Similar confusion is encountered wherever phonology is discussed; small wonder that when one looks to the appropriate section of the "Summary Presentation of the Problem and Conclusions" (335-369), one finds that "it is impossible to mention here all the formal [= stem phonology] types established in this work" (347)! The only solid morphophonemic criterion seems to be the one which states that polysyllabic nouns which drop the *-e-* of their last syllable in the oblique cases of the declension take the genitive allomorph *-a*.

The improvised nature of the argumentation may be evaluated from the following "special explanation" of why two nouns of Category 31 always take *-u* (75):

(a) *limbus* 'limb (marked edge of a quadrant)'. By its size and number of dimensions, it is a Class I noun and should take *-a*. Moreover, the *-us* termination of *prymus* 'portable oil stove' was cited as a phonological termination for the *-a* of that noun (67).

Yet the evidence shows that it takes *-u*. Of the several *dei ex machina* available to Westfal, he ignores the possibility of calling *limbus* "a dental stem" and assigning it to *-u*, as he did with *indeks*, *kompas*, *magnes* (73), and picks instead on an analogy with other nouns terminating in *-bus: autobus*, *omnibus*, which take *-u* because of the size of their designata, and *bambus* 'bamboo', which takes *-u* because it designates a kind of matter (i.e. not an object).

(b) *pług* 'plow' takes *-u*, despite the size of its designatum. This is attributed not to the phonological termination of the stem (as, for example, in the case of *log* 'log', p. 74), but to the analogical influence of *dług* 'debt', which takes *-u*, presumably because it is an abstract! We see, then, a capricious choice among several *dei ex machina*, each of them whimsical in itself.

The most interesting phase of Westfal's work should have been the analysis of the fluctuation between *-a* and *-u* within a given noun. But the first step would have had to be a rigorous proof of the fact that we are dealing with really "free" variation; for it is always conceivable that we are witnessing a split of a single case into two separate cases, an *-a* case and a *-u* case. (One is reminded of *čaju* and *čaja* in Russian—archaic "subcases" of the genitive capable of being opposed to each other.) This proof is not available. With occasional exceptions, the nouns are presented out of context and the source is not indentified, so the reader cannot judge for himself. The suspicion is not ruled out that subtle syntactic factors—no more mysterious than Westfal's so-called phonological stem types—may sometimes be operating in the selection of the genitive allomorph.

Westfal's own approach to fluctuation skips this problem and interprets the indeterminacies as the results of tension between *-a* and *-u* criteria (in addition to the second-order tensions between *-a-* favoring and *-u-* favoring geographic and social dialects [360f., 368f.] and apart from the historic trend, marked since 1900, toward an expansion of *-a* [367f.]). Thus, in analyzing the nouns of Category 31 which have fluctuating genitives, he finds the following conflicts:*

* A few additional cases occur which are harder to tabulate.

-*u* Dictated by	-*a* Dictated By Semantic Class	Also Foreign Stem Morphology
Stem Morphology	11	6
Stem Phonology	28	—
Original Semantic Class (in Extensions)	4	—

In principle, this analysis deals with "dynamic synchrony" at its most interesting. But the intersection of criteria cannot be more precise in its formulation than the statements of the individual criteria; and those, as has been seen, leave a great deal to be desired. Thus, if the choice of criteria is itself *ad hoc*, and the notion of stem phonology is confused, the finding of a conflict between stem phonology and "semantic class" is worth little.

In view of the casualness of the procedure, the unstructural nature of some of the conclusions is not surprising. The "Summary" part begins with the low-order semantic criteria for *-a* and *-u*, probably because they are the author's own discovery; but in the really crucial section 128—"The Role of the Most Important Types of Determination"—the possibility of a hierarchy of criteria is not even mentioned, and all we get is a vapid set of statistics. Yet without a hierarchical arrangement of precise rules, it is as good as meaningless to state that 47 % of the non-diminutive nouns which take *-a* do so in virtue of their morphological makeup, 17 % because of their phonology, and the balance because of their meaning (352f.).

The subject of Westfal's book is enormously important, the material valuable. But his failure to understand the general linguistic problem and to visualize the form of proof that would be required has produced analytic results so plastic and slippery that it is impossible to evaluate their accuracy.

Columbia University. URIEL WEINREICH.

ANDRUS SAARESTE, *Eesti keele môisteline sônaraamat.* (Dictionnaire analogique de la langue estonienne), Fascicule I, XVI+ 191 pp. Stockholm: Vaba Eesti, 1958.

Ce sera le dictionnaire le plus volumineux de la langue estonienne : 24 fascicules à 112 pages chacun. L'introduction du premier fasci-

cule explique la naissance de l'ouvrage et les intentions de son auteur.

Le projet a été conçu déjà en 1914, à Helsinki. Ayant feuilleté le *Traité de Stylistique Française* de Ch. Bally et vu découper les dictionnaires de Lönnroth pour un classement analogique, Saareste a eu l'idée de regrouper les éléments du grand dictionnaire de Wiedemann selon leurs rapports idéologiques et d'y ajouter des exemples tirés du parler populaire et de la littérature. Une partie du manuscrit et les fichiers ont été perdus pendant la guerre et remplacés ensuite par des renseignements recueillis dans des bibliothèques suédoises et finnoises, chez des réfugiés ou par des amis en Estonie Soviétique.

D'après l'auteur, l'originalité de son dictionnaire consiste à expliquer assez longuement les mots, les signes, et à compléter ces explications par des citations littéraires, des expressions dialectales, des proverbes, des locutions périmées et plus ou moins oubliées ainsi que des néologismes contemporains. Le dictionnaire alphabétique, plus pratique pour celui qui cherche un mot connu, sépare et isole les éléments qui s'appellent, qui expriment la même réalité. Le dictionnaire analogique tient compte du fait qu'une seule notion peut être exprimée de plusieurs façons. Il souligne les rapports entre les signes et essaie de donner une synthèse. Il présente la langue comme un système. De ce fait, il correspond mieux aux tendances de la linguistique moderne qui s'intéresse aux structures. Une langue n'a pas seulement une structure phonologique, morphologique, syntaxique, mais aussi une structure lexicale : chaque peuple analyse le monde à sa façon. Ce dictionnaire, donc, peut et veut aider à comprendre la mentalité, les sentiments, le cadre et les conditions matérielles de la vie du peuple estonien, en un mot tout ce qui s'exprime dans le vocabulaire.

L'auteur admet que la matière d'un tel dictionnaire est toujours très vaste et dépasse les moyens d'une seule personne. La classification des éléments reste, bien entendu, discutable. Entre les mots abstraits, les verbes, les relatifs, etc., il n'y a pas toujours de limites nettes, ni dans la langue, ni dans la conscience du sujet parlant. Par contre, les choses concrètes (plantes, animaux, etc.) sont faciles à classer, mais beaucoup trop nombreuses. L'auteur a renoncé à une classification scientifique et donne seulement une liste alphabétique des noms les plus connus.

Le vocabulaire le plus courant a été donné sans explications, car l'auteur suppose que le lecteur a au moins quelques notions

élémentaires de la langue estonienne. Il pense, d'autre part, qu'il vaut mieux éviter les explications définitives pour ne pas devenir trop normatif. Les nuances se dégageront du contexte et des exemples. Pour délimiter les « chapitres », les titres sont traduits en français. Impressions à la lecture du premier fascicule : Valeur ethnologique incontestable : les articles *Aas* « nœud », *Ader* « charrue », *Aed* « clôture » etc., paraissent aussi complets que possible. Richesse très appréciable du vocabulaire : tout est très bien expliqué, dans la mesure où une explication est possible ; documentation abondante parfois même touffue ; de temps en temps on risque de s'y perdre. Quelques termes ont été cherchés très loin ; on a du mal à concevoir le « rapport idéologique » annoncé dans l'introduction. De temps en temps des inégalités : *Aed* I « clôture » p. ex. remplit 6 pages ; on y trouve signalées toutes les enceintes, clôtures et haies possibles et imaginables avec description détaillée. *Aed* II « jardin » occupe une seule page et donne uniquement la liste des types de jardins (verger, potager, etc.) presque tous mots composés avec *aed*. Des lacunes aussi : peu importantes certes, mais mal explicables, p. ex. sous *Aabits* « abécédaires », on trouve les noms des lettres ; tout y est, sauf le *r* qui, pourtant, se dit *ärr*, ce qui est exceptionnel à côté de *ell*, *emm*, *enn*. Quelques expressions assez inusitées demanderaient plus d'explications que n'en fournit le contexte, bien que leurs composants soient parfaitement compréhensibles ; je crois qu'un lecteur même avisé ne saura pas où et comment on a pu employer « terre promise » au sens de *aade* « idéal ».

Un des plus grands mérites de ce dictionnaire consiste, à mon avis, dans son effort pour réunir tous les éléments valables de l'estonien, ceux-même qui ont été proscrits par les linguistes comme des mots ou des expressions « dégénérées ou incorrectes ». Saareste présente ainsi l'image de la langue telle qu'elle est parlée par la communauté.

Paris. FANNY DE SIVERS.

The First Cheremis Grammar (1775). A Facsimile Edition with Introduction and Analysis by Thomas A. Sebeok and Alo Raun. 65+138 pages. Chicago: The Newberry Library, 1956. For) the European Continent, Wiesbaden: O. Harrassowitz.) [= *Studies in Cheremis*, volume 3.]

The *Studies in Cheremis* project has, until now, launched the publication of works in pure grammar (*Derivation*, vol. 4, 1956),

folklore (vols. 1, 2, 6) and ethnography (vol. 1). This volume is philological in the traditional sense. The editors have placed at our disposal a clear photolithographed copy of an old and almost unavailable text (138 pp.) and provided it with a useful and extensive apparatus: demographic and historical data (pp. 8-10), linguistic remarks concerning Cheremis and its dialects and a brief history of grammars of the language (10-12), discussions of the authorship and dialect of the *Grammar* (12-17), an etymological word-list (19-48), morpheme index (49-61), a list of misprints in the original text (63-4), and a list of Cheremis works in the Bonaparte Collection of the Newberry Library (7-8). Bibliographer, bibliophile, historian and linguist will greet the rescue of this work. Its greatest value and interest seem to lie in its nature as a document of eighteenth-century Russian humanism.

A list of Corrigenda and Addenda to the commentary (pp. 1-60) may be obtained from the editors at Indiana University.

Helsinki. ROBERT AUSTERLITZ.

G. B. MILNER, *Fijian Grammar.* VII+150 pp. Suva, Fiji: Government Press, 1956.

Ce petit livre, de présentation soignée, expose en une vingtaine de leçons les traits essentiels de la grammaire du fidjien. Après un court exposé sur la phonologie, l'auteur en vient à la morphologie. Il oppose dès l'abord ce qu'il appelle des « bases » aux « particules ». Les bases sont des mots qui peuvent être employés soit comme « noms », soit comme « verbes ». Les particules employées avec une base permettent de savoir s'il s'agit d'un « nom » ou d'un « verbe » ; d'où la distinction entre « particules nominales » et « particules verbales », auxquelles s'ajoutent des « particules générales », c'est-à-dire employées aussi bien avec un nom qu'avec un verbe. Un énoncé déterminé par une particule nominale est dit « phrase » et déterminé par une particule verbale, « sentence ». Ceci dit, l'auteur étudie une à une les principales particules dans leur rapport avec les bases et s'attache à dégager le rôle qu'elles jouent dans l'économie de la langue. Les pronoms « liés », c'est-à-dire accompagnés d'une base, constituent un des plus importants ensembles de particules. On compte six classes de pronoms liés qui se répartissent en « verbaux » et « nominaux ». Les pronoms verbaux employés devant une base dans une sentence sont des « subjec-

tifs », ceux employés après une base sont des « objectifs », par référence aux fonctions de sujet et d'objet qu'ils remplissent. Quant aux pronoms nominaux, l'auteur s'en sert pour établir quatre classes de « genre » en fidjien. Le genre ici ne fait aucune référence au sexe. On distingue les genres « mangeable », « buvable », « familier », et « neutre », suivant la classe de pronoms nominaux employés pour marquer l'appartenance. Les deux premières catégories sont assez explicites ; par « familier », il faut entendre tout ce qui est en relation étroite avec le « possesseur » (parties du corps, termes de parenté, etc...). Les neutres n'appartiennent à aucune des trois autres catégories.

Sur le plan « verbal », Milner distingue des formes « transitives » et des formes « intransitives », ces termes étant employés avec un sens très particulier. Sont en effet « transitives » les formes dans lesquelles une particule, dite transitive, unit le « verbe » à l'objet ou au sujet. Les formes transitives à suffixe *-a*, qui unit le verbe à l'objet, sont dites « actives » et les formes à suffixe *-i*, qui unit le verbe au sujet, sont dites « passives ». Une forme verbale sans particule transitive et suivie d'une base est « intransitive » ; la base qui suit le verbe « qualifiant » ce dernier. L'étude des particules diverses entrant dans la composition d'un énoncé se poursuit tout au long de l'ouvrage. De nombreux exercices pratiques illustrent les leçons et l'auteur termine son ouvrage par trois appendices consacrés aux termes spéciaux utilisés pour la description grammaticale, aux termes de parenté et à l'intonation.

Le grand mérite de G. B. Milner est de se dégager des méthodes traditionnelles qui ont présidé jusqu'ici à la description des langues océaniennes. Vouloir faire entrer de toute force les faits fidjiens dans des cadres préétablis, et pour l'indo-européen, c'eût été risquer au départ de ne pas rendre compte de faits importants et originaux. Ajoutons que c'eût été renoncer du même coup à dégager la structure de la langue, le système selon lequel elle fonctionne. C'est donc à l'intérieur même du fidjien que l'auteur a cherché des critères de classement en renonçant à adopter nécessairement les catégories traditionnelles. S'il a été conduit à emprunter certains termes à la grammaire des langues indo-européennes pour définir ses catégories, c'est soit par commodité, soit parce que ces termes définissaient des faits semblables. Par contre, l'auteur n'a pas hésité, en cas de besoin, à créer des termes qu'il définit toujours avec précision.

Si le système de la langue ne se dégage pas tout de suite et clairement de l'ouvrage de G. B. Milner, c'est que le but poursuivi

parl'auteur est avant tout didactique ; une présentation structurale bien coordonnée a été ainsi sacrifiée nécessairement aux fins pratiques. L'étude attentive de l'ouvrage permet toutefois d'y remédier, tout au moins en partie. Ceci dit, certaines observations s'imposent concernant surtout des points de méthode. Tout d'abord, l'auteur ne nous paraît pas assez explicite dans son classement des éléments lexicaux. Il nous dit en effet, à propos des « bases », qu'un grand nombre de mots fidjiens « peuvent être employés comme nom ou comme verbe », ce qui laisse penser que ce ne sont pas tous les mots qui sont ainsi « bivalents ». On ne nous dit pas non plus si tout ce qui n'est pas « base » doit être considéré comme « particule ». Nous sommes tentés de croire pour notre part qu'à côté d'une catégorie du type « base » il en existe d'autres qui ne répondent pas au même critère combinatoire. Ceci nous amène à imaginer une distribution du type suivant, très fréquent dans les langues austronésiennes (nous gardons par commodité les critères de Milner) :

1) des lexemes admettant tour à tour une « particule nominale » ou une « particule verbale » (soit les « bases ») ;

2) des lexèmes n'admettant que des particules nominales ;

3) des lexèmes n'admettant que des particules verbales ;

4) des lexèmes n'admettant aucune de ces particules.

A titre d'exemple, un lexème comme *levu* « grand » peut-il constituer indifféremment, suivant la conception de l'auteur, une « phrase » ou une « sentence », c'est-à-dire être sous la dépendance directe d'une particule nominale ou d'une particule verbale? Peut-on avoir *e levu* (sentence) / *na levu* (phrase) comme on a *e lako* / *na lako?* On aimerait plus de précision sur un point aussi important puisque en fait il s'agit bien du classement de base des éléments lexicaux.

On est un peu gêné d'ailleurs de voir l'auteur parler de « nom » et de « verbe » là où il conviendrait de s'exprimer en termes de « fonction ». En effet, il nous dit que les bases « peuvent être employées comme nom ou comme verbe » ; or un lexème est un nom, un verbe ou, comme c'est le cas pour les bases, quelque chose tenant de l'un et de l'autre et constituant ainsi une catégorie particulière. C'est là, pour le lexème, un caractère intrinsèque et synchroniquement immuable. Les lexèmes noms, verbes, ou autre rempliront dans l'énoncé des fonctions déterminées et marquées par des procédés divers que l'analyse permet de détecter. Ils seront

donc employés comme sujet, prédicat, objet, etc., mais non pas comme « nom » ou « verbe ». L'auteur sur ce point semble avoir été encore trop influencé par l'optique commune aux grammairiens des langues indo-européennes.

Il nous semble, que Milner n'a pas poussé assez avant son analyse et l'imprécision qui demeure malgré ses efforts serait due à ce qu'il n'a pas établi au préalable une classification rigoureuse des « particules » dont il se sert pour distinguer le « nom » du « verbe ». Un classement de ces particules aurait pu être tenté sur deux plans différents. Le premier strictement formel permettrait de distinguer les particules proprement dites, c'est-à-dire indépendantes, et les affixes. Le second classement, cette fois « fonctionnel », aurait conduit à distinguer d'une part les éléments servant à ce que l'auteur appelle « l'extension de la base » et que nous appelons des « dérivatifs », d'autre part des éléments marquant proprement un rapport précis entre deux termes de l'énoncé et que nous appelons des éléments « fonctionnels ». Le problème des relations entre « particules » mériterait aussi d'être examiné sous l'aspect combinatoire et donnerait sans doute lieu à un classement en éléments « combinables » et éléments « exclusifs ». De telles méthodes de classement auraient eu pour effet d'éviter de classer sous une seule et même rubrique des éléments occupant dans le système des situations essentiellement différentes, tant du point de vue formel que du point de vue fonctionnel.

La définition que Milner nous donne des « particules nominales » et des « particules verbales » ne nous satisfait d'ailleurs pas. Il dit en effet que « la particule nominale employée avec une base forme une phrase », de même « la particule verbale employée avec une base forme une sentence ». Passant à la définition de la phrase et de la sentence il dit que la phrase (la sentence) est un énoncé d'une base et d'une particule nominale (verbale), ce qui donne :

A (particule nominale)+B (base) = C (phrase) et :

C (phrase) = A (particule nominale)+B (base) etc...

si bien que nous n'avons de définition rigoureuse et satisfaisante ni pour les particules ni pour les énoncés précités. L'existence de « particules générales » complique en outre l'analyse et il paraît arbitraire de considérer systématiquement comme « phrase » un énoncé sous la dépendance des dites particules.

Nous avons vu que l'auteur considère comme « transitives » les formes à suffixe par opposition aux formes sans suffixe, dites « intransitives ». Ainsi nous aurons :

1) *eratou gunu / eratou gunuva*
ils boivent / ils boivent

2) *eratou gunu yagona / eratou gunuva na yagona*
ils boivent du kava / ils boivent le kava

On peut se demander si dans le cas des formes à suffixe, il ne s'agit pas plutôt d'une certaine spécification marquée dans le procès lui-même ; ceci nous paraît d'autant plus vraisemblable que l'auteur nous précise d'une part que les formes à suffixe peuvent être employées sans objet exprimé ; d'autre part que l'objet exprimé après une forme « transitive » est nécessairement précédé de l'article *na ;* cet article étant lui-même une marque de spécification. Le fidjien aurait ainsi la possibilité de délimiter dans le procès lui-même l'extension plus ou moins grande de l'action.

Pour les pronoms on distingue entre autres une classe de « nominaux liés » dont l'auteur se sert pour classer les « bases » en quatre classes de « genre » indépendant du sexe et indiquant une « catégorie grammaticale ». Il y aurait lieu de présenter plus clairement cette classification comme une classification sémantique et non morphologique. Ce type de distribution des sémantèmes est une caractéristique des langues mélanésiennes (généralement deux classes) à l'intérieur du groupe austronésien et c'est régulièrement la façon de marquer « l'appartenance » qui y sert également de critère au classement sémantique. Il serait par ailleurs intéressant de relever les faits fidjiens qui se retrouvent dans les langues indonésiennes, en particulier dans le domaine des affixes. Les nombreuses remarques qu'on pourrait encore faire nous entraîneraient trop loin ; mais elles ne feraient que démontrer l'intérêt que l'ouvrage de Milner présente pour la linguistique océanienne et plus généralement austronésienne.

Paris, C. N. R. S. J. VERGUIN.

A. A. CENSE et E. M. UHLENBECK, *Critical Survey of Studies on languages of Borneo.*, 82 pp. La Haye : Martinus Nijhoff, 1958.

Cet ouvrage est le second fascicule de la série bibliographique consacrée aux études de linguistique indonésienne et publiée sous les auspices du « Koninklijk Instituut voor Taal-, land- en volkenkunde » de la Haye. Cette collection est appelée à rendre les plus

grands services aux chercheurs qui se spécialisent dans les langues du groupe austronésien.

Les auteurs ne peuvent ici que noter, avec regret, l'absence presque totale d'études linguistiques sérieuses en ce qui concerne l'une des quatre plus grandes îles de l'archipel indonésien. Les documents dont nous disposons se réduisent à deux ou trois dictionnaires de valeur inégale, des textes épars et quelques listes de mots, ainsi que des vocabulaires comparatifs. Tout cela est en général l'œuvre de pionniers, de missionnaires ou d'administrateurs, sans formation linguistique.

Les auteurs s'attachent toutefois à nous donner, dans la mesure du possible, une idée de la situation linguistique de l'île.

La pénétration du malais le long des côtes et des principaux fleuves a exercé une influence considérable sur les langues locales, à tel point qu'il semble justifié aujourd'hui de parler de « dialectes malais » dans ces zones maritimes et fluviales. Ces dialectes ont reçu dans l'ouvrage le nom des régions où on les parle (Sarawak Malay, Brunei Malay etc.), exception faite du parler des « Sea Dayak » (les « Dayak de la Mer ») souvent appelés *Balang Lupar* ou *Iban* et qui se rencontrent en de nombreux points côtiers de Bornéo britannique ou indonésien.

De son côté, le Bugis de Célèbes a pris pied sur la côte Sud-Est de l'île et, en de nombreux points côtiers ou fluviaux, des chinois ont introduit leurs dialectes. Si bien qu'en définitive et normalement les langues le moins contaminées doivent se trouver à l'intérieur de l'île.

Mais la distinction entre « langues » et « dialectes » est ici bien délicate à établir, faute de matériaux valables. Une telle distinction exigerait au préalable une étude des différentes structures menée parallèlement à des études lexicologiques, ce qui est pratiquement impossible actuellement. De même la localisation et la délimitation se heurtent à des difficultés faciles à imaginer. Aussi ne faut-il pas attribuer une valeur absolue à la répartition en vingt-quatre groupes que les auteurs n'adoptent que par commodité et qui diffère de toutes celles proposées jusqu'ici. Seules des études sérieuses pourraient, dans l'avenir, nous donner une distribution plus précise et plus conforme à une réalité que nous sommes loin de connaître.

Nous devons être reconnaissants aux auteurs de nous présenter dans leur bibliographie, outre les ouvrages linguistiques, les œuvres de caractère ethnographique les plus marquantes. Par contre, il aurait été bon, dans la partie purement bibliographique, de rappeler

par des titres, les divisions adoptées dans le texte, ce qui en faciliterait le maniement pour quelqu'un qui désire se rapporter à une langue ou à un dialecte donnés. De même on aurait peut-être dû, sur la carte finale, surimprimer en gros caractères les mêmes divisions, étant bien entendu qu'on ne leur accorderait qu'une valeur relative.

Paris, C. N. R. S. J. VERGUIN.

FRANCIS MIHALIC, S. V. D., *Grammar and Dictionary of Neo-Melanesian.* XXI+60+318 pp. The Mission Press, S.V.D., Techny, Illinois, U.S.A., 1957.

The aim of this work is utilitarian and twofold: to provide a reliable guide and book of reference for all those—Europeans and Melanesians alike—who use or wish to use Melanesian Pidgin, and to standardize the written form of that language. "This is the first book published in the officially approved standard orthography of Neo-Melanesian (Pidgin English), which was so many years in the making. This, being a first edition, is necessarily faulty. But it is hoped that the many in government and missionary service who are conversant with the language and its local variations will come to our aid in preparing a better second edition" (ix).

After a brief Introduction by Robert A. Hall, Jr. (v), and a Foreword (vii-x) in which the author tells us: "For my own part, I am looking forward to the day when Neo-Melanesian and this book will be buried and forgotten, when standard English and the Oxford dictionary will completely replace both", but that "meanwhile you and I must be realistic", there is some explanation of the approved orthography (xii-xxi) and a comparative table showing the symbols employed by this and nine other Neo-Melanesian (here after abbreviated as NM) orthographies for each "phoneme or cluster". This is followed by the Grammar (1-60), and, with new pagination, the Dictionary proper (NM-English 1-165, English-NM 167-286) and various Word Groups (referring to anatomy, disease, theology, etc.; 287-318). Where necessary, references will be distinguished by a G or a D following the Arabic numerals.

Phonology is not dealt with; but we are told that "the orthography is the phonemic orthography determined by Dr. Hall,

modified by three extrinsic, non-linguistic factors: cultural pressure ("...a strong opposition to any letters, diacritics, or other usages which are not standard English practice, particularly Australian and British English practice."), selection of one dialect ("that spoken by rather older Melanesian speakers at Madang") and practical school experience. The extent and consequences of this "modification" are not stated at all clearly; but may perhaps be summarized as follows. NM is used today in a number of dialects and varieties by nearly a million people, for a minority of whom (how many we are not told) it is the first language or mother-tongue; while for the others, Europeans and native speakers of one of the 300-odd languages extant in the Territory, it remains a lingua franca. The "over-all pattern" of these dialects and varieties calls for graphic discriminition of thirty-two segmental phonemes (11 vowels and 21 consonants); but, for reasons given above, this was found to be unacceptable. Therefore: "The dialects of Neo-Melanesian spoken by non-native speakers have been excluded from the data serving as a basis for the orthography, resulting in a reduction in the total number of phonemes which have to be considered for orthographical purpose." The approved orthography retains the following symbols : *m p b f v w, n t d s l r, j* (voiced affricate) *y* (not listed on pp. xvi-xvii, but employed throughout the Grammar and Dictionary), *ng* (velar nasal) *k g h*, and *a e i o u.*

It would seem that Fr. Mihalic does less than justice to this orthography, in so far as the "standard" dialect is concerned, when he writes: "To be phonemically exact, about ten different vowel sounds would have to be symbolized, which would render the orthography unwieldy, and would conflict with what was said above under the heading 'Cultural Pressure'. The five ordinary vowel letters only of English have therefore been used, as follows: The *a* as in 'arm', e.g., *papa* 'father'; as in 'hand', e.g., *man* 'man'." But here an editorial note comments: "The usual pronunciation for *man* has the same 'a' as *papa*. What is meant by adding here the anglicised vowel (æ) in 'man' is that whenever such an *æ*-sound occurs, it is written *a*." Other examples suggest conditioned variation (checked vs. free syllables); so, *liklik* 'little' is said to have the *i* of Eng. *hit*, *mipela* 'we' that of Eng. *machine; dok* 'dog' to have the *o* of Eng. *hot*, *nogat* 'no' that of Eng. *old*. If by "The *u* as in 'tulip', e.g., *susu* 'milk'" it is wished to indicate a simple, high back rounded vowel, some such word as 'rude' or 'rule' should be substituted for 'tulip', which has [ju] in standard Australian and British usage.

If, as seems likely, the orthography is at least quasi-phonematic for the chosen dialect, some interesting facts emerge. The *b*, *d*, *j*, *g* and *h* do not occur as word-final, where the corresponding sounds of the English models are replaced by *p* (*rap* 'rub'), *t* (*hait* 'hide'), *s* (*kabis* 'cabbage') and *k* (*bik* 'big') respectively. In order to decide whether this is true of syllable-final within the word, we should require data on juncture (which are not given); but it is interesting to note that intransitive *hait* 'hide' retains the voiceless stop when rendered transitive by the addition of the objective suffix *-im*, transitive *haitim* 'hide'; whereas transitive *subim* 'shove, push', whose stem does not occur as a free form, has a voiced stop before the same suffix. Any vowel or any consonant (not excepting *ng;* so *ngong* 'a deaf-mute') may occur as word-initial; although *f*, *v* and *j* appear to be infrequent in all positions, and to be replaced or replaceable in many words by *p* (*paia* 'fire', *pis* 'fish', *lip* 'leaf'), b (*subim* 'to shove') and *s* (*saket* 'jacket') respectively. Mihalic says (99 D) that wherever *p* and *f* are interchangeable, the former letter is written; and although he seems to be not altogether consistent in this usage (see the following paragraph for an example of this inconsistency), such apparently minimal pairs as: *pul* 'oar, paddle, fin, wing' and *ful* 'fool', *fut* 'foot' and *vut* 'derris root', *sem* 'shame' and *jem* 'jam' (cf. also *paip* 'pipe' and *faiv* 'five', *bilas* 'flash' and *viles* 'village') seem to show that *f*, *v* and *j* are in some cases distinctive. Finally, we are told that *b*, *d* and *g* are commonly prenasalized (as is the case in many Melanesian languages); but this, being non-distinctive, is not shown except for a few words in which the usage has been generalized and where a homorganic nasal is written before the stop, as in *tambu* 'taboo', *sindaun* 'to sit' or 'to sit down', *in-dai* 'to die' (= *i-*, predicate marker, +*dai* 'die').

Consonant clusters—especially such as are word-initial—are often broken up by the insertion of an "anaptyctic" vowel which varies from *i* to *e* to *a*, and whose presence or absence is said to be "not phonemically significant"; thus *stap* 'be located' will often sound like *sitap* or *satap; flai* 'to fly' like *filai* or *pilai*, etc." And in a footnote the Editor adds: "Nor is it accented. An exception to this rule is the word *pilai* 'to play', which always has its stress on the first syllable" (xviii). But if the same speaker has [pi'laɨ] 'to fly' and ['pilaɨ] 'to play', then stress—and stress alone—distinguishes these two verbs. Moreover, from a phonological point of view (on the principle that "once a phoneme always a phoneme"), the first vowel of the former word should be written

no less than that of the latter, or of *bilip* [bi'lip] 'to believe' and *pulap* [pu'lap] 'full'.

Stress (but neither pitch nor juncture) is dealt with in the first pages of the Grammar (2-5). No word has more than one full stress. Words containing any one of half a dozen adverbial suffixes (*-áp* 'up', *-áut* 'out', *-ewé* 'away', etc.) always have stress on that suffix (shown here by the acute accent), as in *karamápim* 'to cover (something)', *singáut* 'a call, cry or sound', *ronewé* 'to run away'. The objective suffix of transitive verbs, *-im*, never bears stress. Otherwise, most words have stress on the first syllable, as in *bikmausim* 'to scold (somebody'); although exceptions—some hundred of which are cited—are by no means rare (so: *biksólwara* 'high sea', *papamáma* 'parents', *pikinini* 'child', *olgéra* 'all', *nambawán* 'the main, principal, very best', *lumás* 'very', *husét* 'who', *nogát* 'no'). Phrases such as: *namba wan* 'first', *no gat* 'not have', *klos meri* 'dress' and *haus pepa* 'office' are said to be readily distinguished from compounds such as: *nambawan* and *nogat* (see above), *doktaboi* 'native medical orderly' and *smolmama* 'aunt', by the former's having two full stresses, the latter only one.

At the outset of the Grammar (2) we are warned that : "The following pages are an attempt to classify the usages of Neo-Melanesian as it is spoken today... in a framework of layman's terms." And again on p. 13: "Although the expert linguist might define the Neo-Melanesian adjective (as well as other 'parts of speech') quite differently from the usual layman's sense, it is sufficient for our non-technical purpose to speak of adjectives, adverbs, etc., as we do." I must admit that I found this "layman's" terminology rather confusing in some cases; as, e. g. when "*milupela*—we two, the two of us (excluding the listener)" is classed as an "inclusive personal pronoun", while "*yumi*—we, us, you and I (including the listener)" is not (9); or when *lukluk (long)* 'to look (at) ' is called a transitive verb although it "requires a preposition before its object", in the same way as *luklukim* 'to watch', *lukim* 'to see', or *gat* 'to have' (*meri i-gat bel* 'the woman is pregnant'—lit. 'has belly'), which do not (24). Also, it seems uneconomical to classify *wet*, as in *rot i-wet* 'the slippery road', as a predicate adjective (15), but *lait*, as in *san i-lait* 'the sun shines', as an intransitive verb (22); the two constructions being quite analogous, and the difference in form of translation arbitrary when cited out of context. On the other hand, this very profusion, which may or may not simplify things for the layman to whom

the book is addressed, should enable the interested linguist (who is not considered) to make his own analysis.

The personal pronouns are, for the first person: *mi* (singular), *mipela*, *mitupela* and *mitripela* (plural, dual and trialis exclusive), *yumi*, *yumitupela* and *yumitripela* (plural, dual and trialis inclusive). And analogously, for the second person: *yu*, *yupela*, *yutupela*, *yutripela;* and for the third: *em*, *ol*, *tupela*, *tripela*. There is no distinction of either gender or case, except that *en* replaces *em* as goal after the prepositions *bilong*, *long* when the meaning is 'it' (rather than 'him' or 'her'). Possession is expressed by the same pronouns, or by a noun, preceded by *bilong* 'of', as in: *het bilong mi* 'my head', *het bilong ol* 'their heads', *het bilong papa* 'father's head', etc.

The function of the "predicate marker", *i*, is not clear. Because of such constructions are: *Adam i-man* 'Adam is a man' and *papa i-yes long mi pinis* 'father agreed with me', one is tempted to regard it as a verbal particle, or even a verbalizer. But that this would be wrong appears from the fact that it is never employed after first or second person singular, *mi* or *yu* (*mi man* 'I am a man' *yu meri* 'you are a woman'), may be omitted optionally after *mipela* or *yupela*, and is employed as well with words having only verbal function as with others (*em i-go pinis* 'he went, he has gone'). Moreover, it is omitted after *em* used as a demonstrative (*em bilong mi* 'that's mine'), and in certain cases of inversion; compare: *husel i-papa bilong yu?* and *papa bilong yu husel?* both translated by 'who is your father?'

Verbs which may be employed either with or without the objective suffix *-im* are transitive in the former case (*em i-boilim wara* 'he boils the water'), intransitive in the latter (*wara i-boil* 'the water boils'); though some of the latter may take an indirect object, as in the case of *bilip long* 'believe in' compared to *bilipim* 'believe'. All verbs containing this suffix are transitive; and some do not occur without it, e.g. *givim* 'to give'. Some verbs cannot take the suffix *-im;* and while most of these are intransitive (so *kam* 'come'), a few are or may be transitive (so *gat* 'to have', *pilai bal* 'to play ball', but *pilai long* 'to play with').

The Dictionary contains some 1500 separate entries in the NM-English section, and more than twice as many in the English-NM section; but this figure does less than justice to the former's richness, since it does not account for a very large number of homonyms, extended meanings and diverse usages listed and illustrated. Thus, under *hat* are given homonyms meaning

'hard', 'heart' and 'hat' respectively; the first used as in *dispela wok i-hat tumas* 'this work is very hard' or in *hatpela taim* 'hard times', the second in reference to the card suit or, interchangeably with *klok*, to the organ (*hat i-meknois*, or *klok i-meknois* 'the heart beats'), while the third is applied to any type of headgear (*hat bilong king* 'crown') and enters into the expressions *kisim hat* 'to be made a government official' and *tekewe hat* 'to remove one from office'. Incidentally, the liver *(leva, lewa)* and not the heart is regarded as the seat of the affections; so that a boy may write to a girl: *yu leva bilong mi* 'you are my sweetheart'. On the other hand, there is probably only one word *skin*, though it may refer to: (1) 'skin, hide, pod, husk, shell, rind, etc.' as in *skin bilong kiau* 'egg-shell', *skin bilong pas* 'envelope', *skin bilong pilo* 'pillow-case', (2) 'a body, a corpse, one's person' as in *planim skin* 'to bury a corpse' and *skin i-les* 'to be tired, to be lazy', and (3) 'the genitals' as in *pilai long skin* 'to play with oneself, to masturbate'. And though they are historically two different words, *pris* 'priest' and *pris* 'preach' or (when used as a noun) 'sermon' are most probably regarded as identical by native speakers today.

The non-equivalence of words belonging to different languages is brought out strikingly in several instances. For example, *susa* (or *sisa*) does NOT mean 'sister', but (1) 'sibling of oppposite sex', (2) 'relative of opposite sex and approximately same age or younger', or (3) 'close friend of opposite sex of approximately the same age or younger'. And analogously, *brada* refers to a 'sibling of like sex, etc.' — that is, to a woman's sister as well as to a man's brother. The word *kokeru* refers to (1) 'a cock (domestic fowl)' or to 'the clitoris'; while *kok* refers to (1) 'the penis' or (2) 'a small tap or pet-cock'. Even such apparently unambiguous words as *yes* 'yes' and *nogat* 'no', when uttered in reply to negative questions, are employed in a way that is contrary to European usage; so, *yu no lukim em?* 'didn't you see him?' may be answered by: *yes, (mi no lukim)* 'no, (I didn't)', or *nogat, (mi lukim)* 'yes, (I did)', the words in parentheses usually being omitted. Yet the Melanesian's affirmation or negation of the negation explicit in the question is at least as logical as our own way of looking at the matter.

About nine-tenths of the basic vocabulary and two-thirds of the total vocabulary appear to be of English ancestry; though many words are employed quite differently from their models (cf. *pisin* 'bird', *kom* 'horn', *gras* 'hair', *slip* 'lie' as well as 'sleep', whose models presumably were Eng. *pigeon*, *comb*, *grass*, *sleep*). Words

taken from various native languages probably come second in importance; such are, e.g., *diwai* 'wood or tree', *kiau* 'egg', *susu* 'milk or breast', *yau* 'ear', *kaikai* 'eat', *(han)kais* 'left (hand)', *arere* 'edge or border', *butoma* or *bilono* 'navel'. The German contribution appears mainly in the cultural sphere: *bangk* 'bench', *belen* 'pray', *blaistik* 'pencil', *bogen* 'arch', *esel* 'donkey', *esik* 'vinegar', *gumi* 'rubber', *haiden* 'heathen', *hebsen* 'peas' (Ger. *Erbsen*, not "*Hebsen*" as Mihalic has it), *hobel* '(carpenter's) plane', *maisel* 'chisel', *slos* 'lock', *srang* 'cupboard', *supkar* 'wheelbarrow or push-cart', *lepik* 'carpet or rug'. (The last example but one suggests that transitive *subim* 'to shove or push' may have had as model for its stem Ger. *schub-*, as in *Schublade* 'drawer', rather than Eng. *shove*.). To the non-cultural vocabulary it has contributed *blut* 'blood', *brait* 'wide' or 'width', *krum* 'bent, crooked, incorrect', and probably *leva* or *lewa* 'liver, spleen' and *kela* 'bald, bare' (Ger. *kahl* seems a more likely model than Eng. *clear*, suggested by Mihalic, which has given *klia* 'clear, intelligible'). But many other words (e.g.: *han* 'hand or arm', *gut* 'good', *laus* 'louse or flea', *pis* 'fish') might equally well have had a German as an English model. From Romance we may mention *save* 'knowledge', 'to know, to know how, to be able' or 'to do often or habitually', and *pikinini* 'child' or 'young of animals or insects' or 'seed or fruit of trees and plants'.

Neo-Melanesian lacks names and employs circumlocutions for some very common things; so: 'feather' is *gras bilong pisin*, 'claw' *kapa bilong pingga* (*kapa*<Eng. *copper*, *pingga*<Eng. *finger*), 'knee' *skru bilong lek*, 'lip' *arere bilong maus*, 'wing' *pul bilong pisin*, 'dust' *smok bilong graun* ,'fog' *smok bilong bus*, 'wax' *sit bilong binen* (Ger. *Bienen* 'bees'), 'ashes' *sit bilong paia (paia* 'fire'). The preposition *bilong* indicates a close relationship (possession, purpose, origin, characteristic trait or quality, etc.); and *long* one that is less close. The latter can be used for: 'in, on, at, to, from, with, by, about, because, for, during'.

In lieu of texts, translations from the Gospels are provided in a separate volume of 96 pages. Rather surprisingly, these are not written in the approved orthography; perhaps because the latter is still unfamiliar to those for whom the Gospel readings are intended. For the sake of comparison, an extract (John 2.6-10) is given below, I as printed (left hand), and II as it would appear in the approved orthography (right hand).

I

Nau hir long haus i gat siksfelo sospen ston belong wash-wash i stop. Fashin belong ol Yudaman i olsem tasol. Wanfelo, wanfelo sospen inaf long tufelo no trifelo baket. Yesus i kirap tokim ol kuki boi, "Fulimapim water long ol sospen." Na ol i fulapim olgeder. Yesus i tok gen long ol, i spik, "Olrait, nau kichim haf, bringim i go long boskuki." Ol i bringim i go. Boskuki i traim disfelo water i wain nau, em i no save i kamap wer. Tasol ol kuki boi i fulimapim finish, ol i save. Olrait, nau boskuki i singgautim disfelo man i marit nufelo na i tok long em, i spik, "Olgeder man i save givim gudfelo wain pastaim. Na bihaind ol man i fulap finish, olrait, nau tasol em i save givim nambertu wain. Tasol yu fasim gudfelo wain i stop inaf nau."

II

Nau hia long haus i-gat sikispela sospen ston bilong waswas i-stap. Pasin bilong ol Yudaman i-olsem tasol. Wanpela, wampela sospen inap long tupela no tripela baket. Yesus i-kirap tokim ol kukiboi, "Pulimapim wara long ol sospen." Na ol i-pulapim olgera. Yesus i-tok gen long ol, i-spik, "Orait, nau kisim hap, bringim i-go long boskuki." Ol i-bringim i-go. Boskuki i-traiim dispela wara i-wain nau, em i-no save i-kamap we. Tasol ol kukiboi i-pulimapim pinis, ol i-save. Orait, nau boskuki i-singautim dispela man i-marit nupela na i-tok long em, i-spik, "Olgera man i-save givim gutpela wain pastaim. Na bihain ol man i-pulap pinis, orait, nau tasol em i-save givim nambatu wain. Tasol yu pasim gutpela wain i-stap inap nau."

This work by Fr. Mihalic admirably fulfils its objective; and makes one wish that the government of the West Indian Federation would take French Creole or "Patois" as seriously as the government of the Trust Territories takes Neo-Melanesian or "Pidgin", thus justifying the preparation of a similar work for that language.

Dominica, British West Indies. DOUGLAS TAYLOR.

NOTES

QUALITATIVE STRESS IN FRIULIAN

In their interesting exposition of *Friulian Phonology* (*Word* 8.216-23,1952), B. W. Bender, G. Francescato, and Z. Salzmann set up (221) ten vowels in two matching sets of five stressed strong and five stressed nonstrong; the strong vowels /î ê â ô û/ are phonetically tense ['i 'e 'a 'o 'u], while the nonstrong /*i è à ò ù*/ are phonetically lax ['ɪ 'ɛ 'a 'ɔ 'u]. For the unstressed syllables they are then obliged to recognize the neutralization that there obtains by employing 'Prague-style archiphonemes', as they term them (222).

It has never been clear to the present writer just what the status of archiphonemes is with respect to phonemes. For example, do we have here fifteen entities, or do we have just ten after all?

In any event, in the present case it would seem that a more economical solution can be reached through conventional phonemic analysis without appeal to archiphonemes. If we set up for Friulian two qualitative types of main stress, tense /ˆ/ and lax /ˋ/, we may say that the five simple vowels /i e a o u/ are found to occur with either of the stresses or with neither. At the same time we may note in this stress system an interesting instance of what Hockett (*A Manual of Phonology*, § 2321) calls Non-linear systems with zero.

University of Chicago. ERIC P. HAMP.

Towards the end of my paper, "'Marathi English': A Study in Foreign Accent" in *Word* 13:2 (August 1957), I suggested that "some of the structural tensions of English proper may reveal themselves, as it were, when subjected to this kind of 'treatment' ", i.e., being spoken by foreigners. I had in mind such things as my suspicions about the Trager and Smith account of contrasts like *blackbird : black bird* roused by the way Marathi

speakers of English handle this, namely, in terms of juncture (see section 5 of my paper for details).

I would now like to draw the attention of the reader to the baring of this fact as an interesting side corroboration of the main thesis of the paper in the same number by D. L. Bolinger and L. J. Gerstman, "Disjuncture as a Cue to Constructs".

Poona, India.
Cornell University, Ithaca, N. Y. ASHOK R. KELKAR.

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LINGUISTIC CIRCLE OF NEW YORK

Program Committee Report

The speaker programs of the monthly meetings of the Circle during the academic year 1957-8 were as follows:

October 19	John B. Newman (Queens College) Leo Pap (State Univ. Teachers Coll., New Paltz)	"Two Conceptions of Semantics (with Special Reference to de Saussure)"
November 9	William E. Welmers (Hartford Seminary Foundation) Ephraim Cross (City College of New York)	"Types of Tone Structure in Language" Report on International Congress of Linguists — Oslo, 1957
December 14	Seymour Chatman (University of Pennsylvania):	"Sign Systems and Poetic Systems"
January 11	Symposium-Discussion: "Desiderata for a Basic Course in Linguistics" Participants: Irving S. Carton (New York City), John P. Hughes (St. Peter's College, Jersey City), Aert H. Kuipers (Columbia University), B. Hunter Smeaton (Polytechnic Institute of Brooklyn). Moderator: M. D. Berger. This meeting was held jointly with the Columbia University Linguistic Circle.	
February 8	William N. Locke (M. I. T.)	"How to Make a Computer Translate"
March 8	Joseph H. Greenberg (Columbia University)	"Some Historical Problems in African Languages"
April 12	William G. Diver (Columbia University)	"A New Laryngeal in Indo-European"

These meetings were held regularly on the second Saturday of each month from October through April. The meeting place was the auditorium of the French Consulate, 934 Fifth Avenue at 75th Street in New York City; the time — 2 P. M.

Our third annual spring conference was held on May 10, 1958, in the Hunter College High School auditorium, 930 Lexington Avenue at 68th Street, New York City. This was a change in two respects over previous years: our first two conferences had been held in April at our regular meeting place.

This time we felt that the conference ought to be put at the end of the academic season as a climax to the year's activities and that a more spacious meeting hall in an academic atomsphere would lend dignity to the occasion.

The Conference program follows:

1. George W. Grace, Columbia University:
   'Origin of Malyo-Polynesian Initial Nasal Clusters'
2. E. Adelaide Hahn, Hunter College:
   'Naming Constructions in Old English'
3. John P. Hughes, St. Peter's College:
   'Dating the Beginning of Modern Irish'
4. Judah A. Joffe, École Libre des Hautes Études:
   'Mutual Borrowings in Yiddish and Slavic'
5. Mel O. Most, Language Research Project:
   'Reduplicate Influence on English Word Forms'
6. Rebecca R. Posner, Wesleyan and Oxford Universities:
   'On Consonant Dissimilations in Romance Languages'
7. Marshall D. Berger, The City College of New York:
   'The English Functional Syllable'
8. Robert A. Fowkes, New York University:
   'Some Phonological Aspects of Welsh-English Bilingualism'
9. Paul L. Garvin, Georgetown University:
   'Aspects of Machine Translation, with English-Russian Examples'
10. Theodore M. Gutmans, United Nations:
    'British-American Variants in Standard Radio Speech'
11. G. Kingsley Noble, Hunter College:
    'Affiliations of Taino (Haiti) to Island Carib and Other Arawakan Languages'
12. George C. Pappageotes, Columbia University:
    'A Language with Two Standards: Modern Greek'

The conference was attended by a hundred persons, our best showing to date and a tribute to the hard work and competent leadership of our conference organizer, Mel Most.

Our kind thanks are due also to Elliott V. K. Dobbie, who chaired the conference throughout the long day with unfailing good humor, and to John P. Hughes, Jeanne V. Pleasants, Judah A. Joffe and Fredrick Jungemann for all the many services, great and small, that they rendered throughout the year.

MARSHALL D. BERGER

The City College of New York
Program Committee Chairman.

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